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The Inheritance of Fecundity in Fowls

OSCAR SMART

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THE INHERITANCE OF FECUNDITY IN FOWLS

: By OSCAR SMART :

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TO

MY FATHER AND MOTHER

INTRODUCTION

By

The Editor of "The National Poultry Journal."

I do not pen these lines as an introduction to the science of breeding, but as an introduction to the work of the author of this book.

It was, I think, about 1912 that the name of Oscar Smart first became recognised as an authority on Breeding, but his work had been in preparation since his boyhood. He was a life-long invalid, rarely out of his own garden, and this fact, together with his love of Biology, provided him with opportunities to study and experiment in the problems of heredity. He began his work as a schoolboy, not merely with the boyish love of animal life, but as a student of cause and effect. Successively he bred and experimented with Canaries, Pigeons, and Poultry. He studied the great masters, such as Darwin and Mendel; he applied their teachings to his own stock; sometimes his work corroborated theirs, sometimes it did not. He formed new theories which in practice often proved wrong but more often right. Chiefly his work lay in the study and application of Mendelism, but in some important details he had cause to reject that great Biologist's theories. About 1910 he felt fitted to offer his work to the public, and, as is so often the case with really great men, it was rejected. In latter years, when his work had won public recognition, he was fond of telling how he first won literary fame. Two articles were submitted to the Editor of a well-known poultry paper. The one dealt with the washing of exhibition fowls, the other dealt with the inheritance of fecundity, and represented fifteen years of solid study and experimental work. The former was accepted and paid for, the latter declined. It was a bitter blow, but serves to show how little the science of breeding was understood so recently as 1910. In 1912, articles were submitted to " Eggs," whose Founder, Mr. Randolph Meech, was quick to appreciate their value. It is he to whom the industry is indebted for bringing Oscar Smart's work to public notice. From that year until about eighteen months before he died in June, 1919, he contributed weekly articles to that paper. In " Breeders' Queries " and " Science Jottings " he gave us some of the finest of his work, and it is my hope that it will be collected and issued in book form. From its first issue he became one of the contributors to the " Illustrated Poultry Record "—a fine English monthly, now unfortunately defunct. A large quantity of unpublished manuscript was left in the Editor's hands at the time of Mr. Smart's death. This has not yet been recovered. Wider publicity was given to his work by " Poultry World " in 1919. This paper was then under the Editorship of Mr. J. W Hurst, who fully recognised the value of Mr. Smart's work and gave his readers short weekly articles from this now famous pen. About a year before he died Mr. Smart filled the post of Query Expert to " Poultry World," and he carried on this work right up to the time he passed away.

Reference to the files of Poultry papers, before and after his teachings were practised, will show what a tremendous influence his work has had upon the breeding of poultry. Nowhere is this so evident as in breeders' advertisements. He preached the value of strain, he taught us the meaning and value of stamina, his principles of breeding are advocated and referred to in almost every page of every poultry paper. It is not too much to say that until his work became available, the breeding of utility Poultry was chiefly a matter of chance.

It is not generally known that he mated every year the stock of many of our most prominent farmers. His work in this direction, and the reports he received from correspondents in all parts of the country, gave him a knowledge of our breeding stock unique in the history of the Industry. He knew the weaknesses and strong points of each, and he used his knowledge to such effect that beginners working under his direction built up, in the course of a few years, more reliable stock than that possessed by the originator. It is, I think, true that 90 per cent. of our successful breeders are now working upon the lines laid down in this book. They represent the only reliable method of building up a flock of really high fecund stock.

I discussed Hoganism with him on many occasions. He said that this method of selection was useful in the hands of skilled breeders, if used in conjunction with the trapnest. To him Hoganism was simply a method of culling ; he did not regard it seriously as a breeding operation. During the last few years his opinion has been expressed again and again by the best known and most successful breeders in the country.

In addition to the valuable work on breeding he left behind him, he contributed to the feathered tribe the Mendel fowl and the White Campine. He also contributed largely to the making of that extraordinary breed—the Silkie Croad Langshan. Just before he died he had taken the preliminary steps towards making a Rhode Island Black. This work was never finished, but the three breeds first mentioned are still with us, and it would seem that the Mendel is to become one of our popular breeds.

Mr. Smart's work in connection with the Clubs and Societies is well known. He was the Breeding Expert and one of the Founders of the Scientific Poultry Breeders' Association. The knowledge and labour which he contributed during the early days of its existence did much to make the foundation on which the Society was built. He formed the Poultry Research Society, and until he could carry on no longer the Society was a pronounced success. His greatest honour, and the one he treasured most, came from a Society with which he was in no way connected—The National Utility Poultry Society, which conferred upon him its first Honorary Life Membership. It was a mark of appreciation of a great work, and worthy of the members of our largest Utility Poultry Society.

When he died Mr. Smart left an uncompleted manuscript of a book he intended publishing, under the title of "Inbreeding and Linebreeding Exhibition and Utility Fowls." I read through this MS., and it promised to become a classic, just as the "Inheritance of Fecundity in Fowls" has done. There is an undoubted need for a book on Inbreeding and Linebreeding. The introduction of new blood is a very important matter to the stock breeder, and as yet there is no reliable guide for him. How important Mr. Smart thought it may be gathered from the fact that charts of matings and new groups in the Mendels were left to serve for twenty years after he died.

Oscar Smart was not a rich man. He never could have been. He gave freely and willingly. Possessed by others, his great skill and knowledge might have won fame and fortune. For him they won fame and the grateful thanks of many thousands of poultry keepers in every part of the globe. He made breeds, strains, and businesses, but he could never conduct his own business properly. Sentiment and generosity do not harmonise with business instincts. Anyone could appeal to his sentiment successfully. He was generous to a fault. I have seen him spend days on end working out matings for some poultry farmer or other. Possibly he would charge them 5/-, probably he would do the work free because they were "trying to get on." He never thought of "getting on" himself. His reward came to him in the success of his pupils.

He was a great lover of animal life in all its forms, and his knowledge of it was deep and solid.

It is very gratifying to me to have been associated with the first and now with the second edition of this book. I think its sale has been a record for a poultry book at such a price.

Until some great Scientist gives us some simpler method of obtaining the desired results, I shall build up the productiveness of my stock by the study and application of the methods of breeding taught in "The Inheritance of Fecundity in Fowls."

J. A. WATSON.

Oscar Lunart

AUTHOR'S PREFACE

In giving this book, which contains the results of many years of research work on the inheritance of fecundity in fowls, to the public, I do so with no small amount of trepidation and fear. For I cannot disguise from myself—it is not right that I should disguise from myself—that the art of breeding is a progressive science, so that the work of to-day too often renders the work of yesterday out-of-date. I know that had I written on this self-same subject even so short a period as three years ago, many of my deductions arrived at then I could not have honestly supported now. I am glad I did not write the book then ; I wonder if I am right in yielding to the importunities of my friends in writing it now ! The answer to that question remains for the present in the lap of time.

There are many egg farms in this country yielding excellent profits year after year, while there are many others being run at a loss. It is with the failures that I am principally concerned. Even with the best housing and the best feeding many fowls do not pay their way, and our markets remain open to the foreigner to dump in his eggs to the tune of nine million pounds' worth annually. I ask myself : " Since some farms pay, why cannot all ? " and the more I think about it the more convinced am I that, in addition to housing and feeding for prolific egg production, we must breed for eggs, too. But this is a highly specialised science, and it is not to be expected that breeders can breed the very best class of laying stock unless they be told how. This book is designed to tell the reader how heavy egg production is transmitted and inherited, as it is my firm conviction that, given the requisite knowledge, there is no man in the world equal to the British stock-breeder.

I have tried to write for the man with no scientific knowledge. It is a matter of grief to me that the scientific man writes and works only for science and not for the benefit of his fellow men, and couches his writings in such terms that they are entirely beyond the reach of the average breeder. I have tried to avoid this. But I do not think it would be fair to expect of me that I can reduce the science of breeding for increased egg production to such simple terms that no effort is required on the part of the breeder to get the best my book has to offer him. If anyone expects that, he will be disappointed. I have done what I could in order to make the matter simple, but the reader must do his part, too. I do not believe that anything I have written is beyond the power of even the most untrained to follow, provided he will give it that time and attention which the subject undoubtedly deserves.

Much that is best in this book I owe to the kindness of my many friends. My thanks and acknowledgments are especially due to the following :—

To Mr. George Edwards for sketching and shading the birds with which I have been enabled to illustrate the inheritance of fecundity, and without whose assistance I do not think this work could have been published.

To the Editor of *Poultry World*, who has made all arrangements for the publication of this work, thus relieving me of much worry at a time when I needed such relief most.

To the Editor of *Poultry World* and to my Father, who have been good enough to read over the proofs of my book and to correct its many errors.

To my Sister, who relieved me of all work attached to the advertisement pages, and to whom I am indebted for the exhaustive index at the end of this work.

To those many, many friends—unfortunately known to me only by name—who, learning that I was preparing such a book, wrote me long and helpful letters of encouragement.

Lastly, to all those kind people, who by placing their advertisements in my book, have allowed me to give a better work, and at a much lower price than I had at one time thought possible.

OSCAR SMART.

BONNYBROOK, SUNBURY-ON-THAMES.
September 22*nd*, 1917.

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Key to Diagrams.

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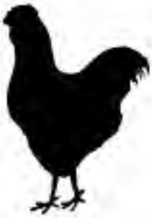

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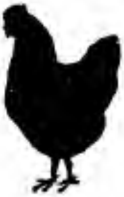
Yearly Record, over 280.



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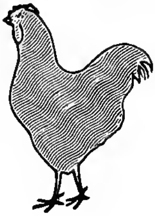

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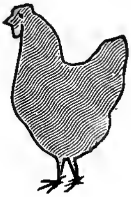

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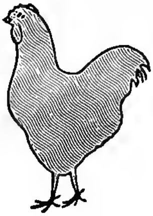

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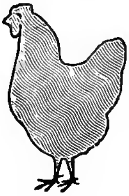

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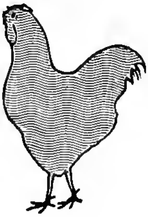

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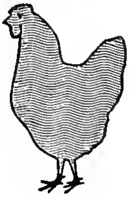

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Yearly Record, 140—199.

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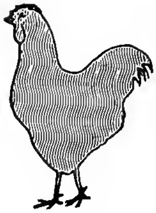
Plate 2.

Key to Diagrams—*continued.*

L 1 TYPES.



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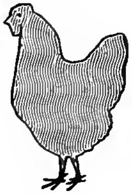

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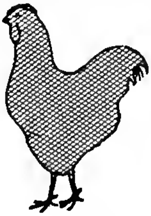

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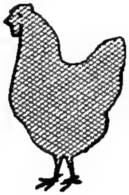

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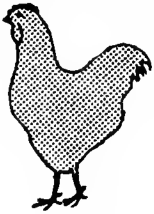

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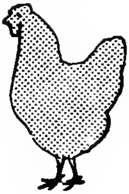

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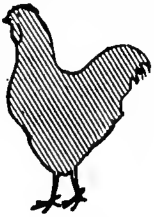

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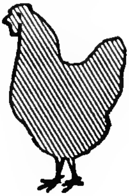

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Plate 3.

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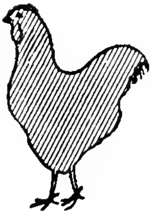
ZERO TYPES.



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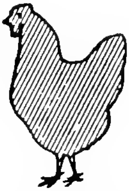

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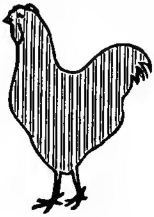

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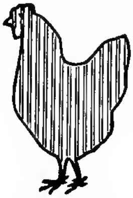

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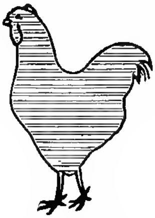

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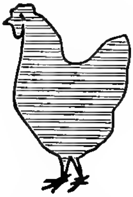

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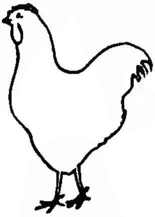

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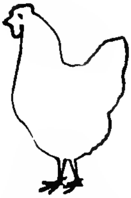

No. 24.
Winter Record, ***Nil.***
Yearly Record, under 30.

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THE INHERITANCE OF FECUNDITY IN FOWLS

WHAT IS FECUNDITY ?

Fecundity means to be fruitful—to bring forth young. In studying the inheritance of high fecundity, it cannot be too strongly insisted upon that we are dealing exclusively with one of the processes of reproduction. It matters not whether we incubate the egg or eat it, the fact remains that it was laid in order to produce a chicken—and not to grace our breakfast table. It follows that in studying the inheritance of fecundity, we are studying the inheritance of the reproductive power and, as we know, this power is of a highly variable nature in different individuals even when of the same parentage, and is subjected to a variety of different influences.

It is affected by light, temperature, altitude, nourishment, disease, stimulation, the development of functions and of organs other than those directly concerned with reproduction, age, inter-breeding, out-crossing, and by heredity. It is as well to bear all these things in mind when studying its inheritance. For each of them possesses the power either to increase fecundation or to check it according, as it seems to me, to the *degree* rather than to the *kind* of influence applied. Thus generous dieting up to a certain point will increase fecundation, but carried beyond that point it reduces it ; to remove the individual to a *rather* higher altitude will have a most stimulating effect upon its egg organs, but to remove it to a *much* higher altitude will speedily cause the organs to degenerate ; a first-cross will frequently cause a great increase in fecundity, while a further cross will more often than not prove disastrous.

In dealing with fecundity, we are therefore dealing with a natural phenomenon which, in addition to heredity, is subjected to a variety of environmental influences, to any or all of which it is particularly sensitive. Let us now consider the egg organs of a hen with a view to determining the anatomical provision for fecund responses to both her heredity and to her environment.

THE ANATOMICAL FACTOR.

The hen is possessed of an ovary in which the eggs mature, and an oviduct in which they are fertilised and down which they travel while being coated with the white or albumen and receiving their encasement of shell. This ovary, when thoroughly healthy, is highly organised ; it consists of a mass of muscular tissue through which run innumerable veins, arteries, and nerves, and in which are embedded hundreds of tiny yellow bladders called *Oocytes.* Of all organs none possesses a greater power to regenerate than does the ovary. After double ovariotomy in humans, where both ovaries have been found cystic, women have been known to bear children ; this is due to a particle of ovarian tissue having been left by the surgeon, and from this particle of ovarian tissue *a perfect ovary with all its parts and functions* has been regenerated.

If we take the ovary from a hen, place it in a shallow dish of clear water, and dissect it with a couple of strong needles, we shall free the oocytes and be able to count a large number of them with the naked eye. We ought to be able to count about 2,000 of them in this manner. Some (those which are nearly mature) will be as big as fair-sized walnuts, and it will be found that they grade down in size to the vast majority, which will be no bigger than a pin's point.

These oocytes are " yolk bladders," they are potential eggs. The yellow membrane encasing the yolk as we know it is the oocyte after it has grown and become distended with yolk. Now the oocytes we shall call the *Anatomical Factor,* because these "yolk bladders" are the embyro eggs, as it were, in the body of the fowl. And here we find ourselves confronted by a very serious problem, *for the anatomical factor bears no relation to the fecund factor,* as can be shown in a very simple way. The highest number of eggs from any recorded hen is just over 1,000 in eight years; the lowest something less than 100 in five years. The dissection of the ovaries of eight months old pullets of highly fecund strains of White Leghorns and of eight months old Indian Game pullets, of low fecund ancestry, showed that each contained approximately 2,000 oocytes. Experimental evidence therefore goes to prove that the anatomical factor is fairly constant for all fowls, and that whatever be the deciding factor in high or low fecundity it certainly is not the number of oocytes inherited.

Nor is there any evidence in support of the view that many (at least two-thirds) of the oocytes are rudimentary and therefore incapable of becoming fruitful, for apparently none of the oocytes show any organic differences. A more probable explanation appears to be that the number of eggs laid depends on the amount of yolk available for charging the oocytes. The more freely this flows the more freely will the bird lay, but no sooner does she begin re-absorbing yolk than does her fecundity decrease until eventually it stops altogether. As will be gathered from these remarks, I personally see no reason why the whole 2,000 oocytes should not be brought to fruition, *provided that a sufficient quantity of yolk were fed and made available with which to charge them.* There is sufficient evidence to show that (1) low fecundity may be due to re-absorption of the yolk; (2) that high fecundity is due to a free and constant flow of yolk into the oocytes, and (3) that every oocyte possesses the power of functional activity. Let us now examine a small portion of the evidence in support of these views.

ACQUIRED HIGH FECUNDITY.

Quite obviously if egg production is dependent upon "yolk flow," it may be affected either from something from within or by something from without, or, still more probably, by both. The first of these is the bird's heredity, and the second its environment, which must be understood to mean and to include all with which it can organically come into contact. Changes effected by inborn variations we call *Genetical*, while changes effected from without we call *Acquired*. It is with these latter kind of changes, as affecting fecundity, that we must first deal.

Yolk is secreted in what are known as *Yolk Glands*, and is conveyed to the oocytes by means of *Yolk Ducts*. These glands are formed by the convolution of a large number of vessels, and they draw their secretions from the blood. The oocytes draw the yolk from these glands through the medium of the ducts, and after becoming fully charged they rupture, and thus being set free from the ovary are deposited in the oviduct. Under certain conditions the functions of these glands may be reversed, when, instead of drawing their secretions *from the blood* and depositing them in the oocytes, they draw the yolk *from the oocytes* and deposit it in the blood. This reverse process is known as *Re-absorption*. The environment frequently affects the action of the glands. Let us enquire how.

The organism makes a double call upon the food it consumes—first it calls upon the albuminoids to make up the waste which is continually going on in the tissue, and at the same time it calls upon the carbohydrates to generate heat. If the albuminoids be slightly in excess of what is required, the excess is absorbed by the yolk glands, converted into yolk, which is then absorbed through the ducts into the oocytes; if the albuminoids be greatly in excess of what is required, after a time they cannot be assimilated, the tissue is robbed of nourishment, the blood is "starved" and begins drawing upon the yolk glands for their secretions, and egg production ceases ; again, if the albuminoids be less than is required the tissue still draws upon the blood, the blood has to draw upon the yolk glands, which, in their turn, absorb any particle of yolk which may be in the oocytes, thus putting a stop to egg production.

To a much lesser extent this is also true as regards the carbohydrates, except that a continuous excess of these will cause an accumulation of fat rather than of yolk. If the carbohydrates be very slightly in excess of what is required, they have a tendency to lessen the waste which is going on in the tissue, " thus releasing " a certain amount of albuminous matter, which the yolk glands absorb, and to this extent egg production is thereby increased. If, however, the carbohydrates be in so great an excess as to cause an accumulation of fat deposits, these, by pressure on the glands, whereby their activity is restricted, cause the egg supply to fall off.

These points may be summarised thus :—

1. By a slight increase of such foods as fishmeal, meat-meal, dried yeast, pea-meal, bean-meal, and similar highly albuminous foods, egg production is accelerated.

2. An excess of such foods decreases production.

3. When the diet is most deficient in albuminoids—say, a diet of oats, bran, rapeseed, swedes, lettuce and vetches—egg production stops.

4. By adding a little maize, barley, or oatmeal to the mash, eggs become more plentiful.

5. By greatly increasing the quantity of these the birds put on fat and the egg supply is checked.

We therefore see that by an intelligent alteration of the feeding we are able to increase fecundation, to decrease it, or to stop it absolutely. Changes brought about in this manner are said to be *Acquired.*

The activity of both the yolk glands and the oocytes which they supply with yolk are easily affected by altitude and by temperature. Either can be used to stimulate or to enervate, for the cells and glands make a rapid response to either condition. Stimulation is, however, only possible up to a certain point ; carried beyond that, it results in degeneration of the various reproductive parts when, as we shall see, fecundation is decreased or becomes entirely suspended.

Speaking broadly, the valleys are enervating and the higher altitudes stimulating. If we bring birds from an altitude of, say, 400 ft. above sea level down into the valley, there will speedily be a most marked decrease in egg production ; if, however, we take them from the valley up to a 400 ft. level, the fecundity will be increased. It might be argued from this that the greater the altitude the greater is the fecundation. This, however, is not so. After the 600 ft. level is reached, the higher up you go the less is the fecundation. An increase in altitude is only effective in increasing fecundation provided you do not go beyond certain levels—beyond those levels not only does the beneficial effect cease, but a positively hurtful effect is produced. We have, then, three distinct facts in respect to altitude and fecundation :—

From the valley up to a 600 ft. level increases fecundation.

Much above the 600 ft. level causes a marked diminution in fecundity.

From a high altitude (not exceeding 600 ft.) down into the valley decreases fecundation.*

These facts are accounted for quite simply. As we have already remarked, both the yolk glands and the oocytes quickly respond to stimulation, and just as quickly become enervated, according to the conditions under which the birds live. These responses are effected through the nerves, which cause greater or lesser activity in the parts concerned in marked correlation to the degree of stimulation or enervation applied.

The different effect of altitude as shown when a bird is taken up to a 600 ft. level, and when it is taken beyond, may be likened to the effect which cold water has on our own skin, provided that we are healthy and that our circulation be good. A cold dip, followed by a smart towelling excites and stimulates the capillary nerves just under the skin, quickens the circulation, and sends a glow of warmth and of health through our bodies. Now, mark this point : if the temperature of the water be still further reduced until it approaches freezing point, its stimulating effect will cease, and instead it will tend to numb the nerves and to lessen their functional activity, thus giving a dead and heavy languor to the body. So it is with altitude and fecundation. So long as the stimulation be within such limits as renders it within the power of the cells and glands to respond, fecundation will increase ; but so soon as it exceeds these limits, and the stimulation becomes greater than the possible response, then so soon will the activity of the cells and glands decrease with (as a result) a corresponding decrease in production.

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*These altitudes refer to the United Kingdom only.

A hot bath in its effect may be compared to taking birds from a high altitude into the valley. It has a " soothing " effect, or, in other words, it decreases the functional activity of the capillary nerves—it " relaxes " them ; hence the sensation of restfulness. The mild, soft atmosphere of the valley has a similar restful and enervating effect upon the oocytes and glands, it makes them less active—soothes them into a kind of partial slumber—and because their functional activity is thus decreased, the egg production of the birds suffers in corresponding proportion.

Among other environmental conditions which affect fecundation in fowls, special mention may be made of the following :—

Temperature.—An increase in the temperature, such as is brought about by the advent of spring, causes an increase in egg production ; but a great *increase* in temperature, such as we sometimes experience in June, July, or August, will decrease production either by throwing the birds into an early moult or by encouraging excessive broodiness. A *very low* temperature checks fecundation.

Light.—A *bright* light increases fecundation; a *dull* light diminishes it. Fowls confined to a dark house never lay well; fowls taken from a dark house and placed in a very light one speedily become far more prolific. Conversely, birds taken from a very light house and placed in a very dark one will temporarily stop laying, and later the fecundity will be found to have decreased.

Damp.—During a spell of wet or damp weather fecundation tends to decrease. A badly-ventilated house is almost invariably damp, except during a long spell of very dry weather, and birds kept in such houses lay but badly.

Parasites.—Both internal and external parasites have a prejudicial effect upon egg production.

Sexual Irritants.—There are many drugs which, by irritating the sexual organs, increase their activity. They cause a period of increased fecundation; but the effects are not permanent, and if persisted in they are liable to cause degeneration of internal parts.

Change of Diet.—To make a total change of diet just as the birds are beginning to "go out of lay" in about the month of July or early in August, will usually give rise to prolific fecundation. The effect lasts from three weeks to a month, and is rapidly followed by the moult.

We have here reviewed in rapid succession a few of those environmental influences which are known to effect fecundation in fowls. We have seen that each in its way acts directly upon the organs themselves, either by (1) nourishing them; (2) causing them to be starved; (3) stimulating them; (4) enervating them; and (5) causing their degeneration. These are all "acquired" changes, because they are brought about, either directly or indirectly, through the environment. Such changes affect the individual, and not the race. They are not inherited. The small and rapidly-diminishing school of Neo-Lamarckians contend that acquired characters may be hereditarily transmitted. I ask them to produce the proof. If their contention be true, they could demonstrate it experimentally

—nothing would be easier. They cannot prove it experimentally. Every effort they have made to do so has gone to prove that the very reverse is the case—namely, that acquired modifications are not inheritable. Therefore we may accept as correct the view that any increase in the fecundation of fowls, which is directly or indirectly due to changes in their environment, *will not be transmitted to the progeny of such birds.*

GENETICAL HIGH FECUNDITY.

Since genetical or " inborn " high fecundity is the only kind of high fecundity which is inheritable (and therefore possessed of a selective value), it is with this class of fecundity that we are chiefly concerned.

The germ-cells are of two kinds—the ovum, or female cell, and the spermatozoon, or male cell. When these two fuse on fertilisation we get the oosperm, or " fertilised ovum." This oosperm contains hereditary factors transmitted to it by both parents, and each of these factors is a highly differentiated entity. There must be no mistake on that point—only in the rarest possible cases do the factors transmitted by both parents blend in their offspring. In the great majority of cases the offspring is like its mother in some characters and like its father in others—it is never like either of its parents in *all* its characters. Among the various factors which may be transmitted by either parent is the fecund factor, and it will prove profitable to consider in the first place what this factor actually is.

We have already seen that the hen contains an ovary, oviduct, yolk glands, yolk ducts, and oocytes, all of which are concerned in egg production. We have also seen that the anatomical factor in egg production bears no correlation to the actual degree of fecundation—on the contrary, we have found that the degree of fecundity is dependent upon the amount of yolk that flows into the oocytes rather than upon the oocytes themselves. Thus, a highly fecund hen differs from a low fecund hen not in the number of potential eggs which she inherits, but upon her power to bring these potential eggs to fruition, this power in its turn depending upon the *freeness* with which the yolk glands can convert the elements which they collect from the blood into yolk and discharge it, through the ducts, into the oocytes. This power is called the *Physiological Factor* in egg production. As we shall shortly see, there are three of such factors.

The physiological factor is both variable and inheritable. Let us first consider its variability. It consists of several parts: (1) the construction of the glands themselves, (2) their number, (3) their size, (4) their functional activity. How far any one of these parts is dependent on one or all of the others we do not as yet know; but it is at least a possible hypothesis that (4) is dependent in at least some measure on (1). There is room here for enormous variation. The possibilities are:—

1. Structural variation.
2. Numerical variation.
3. Variation in size.
4. Variation in functional activity.
5. Variation in the relation of any or all of these to the others.

Hence, the possibilities of variation are enormous, but not greater, I think, than is the variation in production. But apart from the *nature* of the variation, we also have to reckon upon its *degree*, for it is clear that the variation may be either definite (dimorphic) or indefinite (fluctuating).

The exact manner in which the inborn or genetical variation is brought about can only be conjectured. There are possibly many ways. The germ-cells themselves may undergo physiological changes, so that the characters of the organs evolved from these may be correspondingly changed; in the maturation of these germ-cells one-half of the maternal and one-half of the paternal chromosomes are disposed of in the first and second polar bodies, which perish—this may cause variation; both parents contribute to their offspring, and neither are alike, hence the progeny must be different ; again, however far the individual may be removed from any given ancestor, that ancestor's blood is still flowing in its veins, and when we come to consider the millions of ancestors possessed by each individual, we cannot but admit that the possibility of variation in each is endless. Galton, in his " Law of Ancestral Inheritance," gives the following scale of ancestral contributions to the individual :—

From 2 parents.	From 4 grandparents	From 8 great-grandparents	From 16 great-great-grandparents
$\frac{1}{2}$	$\frac{1}{4}$	$\frac{1}{8}$	$\frac{1}{16}$

This ratio is supposed to go on *ad infinitum* for ancestral inheritance, and although its correctness is a matter for the gravest possible doubt, it is at least certain that the ancestral contributions count for something (although probably not in a constant ratio such as Galton suggests), and the greater the difference between the ancestry of the individual parents the greater must be the possibility and range of variation.

The inheritance of some genetical variations is established, while the inheritance of others is in doubt. This matter will come up for further consideration later under the respective headings of " Dimorphic Variations " and " Fluctuating Variations." At present we are concerned with a different aspect of the subject, namely, how a variation comes to be inherited at all. Let us take a specific case.

If AA represents the genetical factor in the germ-cells for a particular degree of functional activity in the yolk glands, and a series of capital A's in parenthesis (AAAAAA) represents the same degree of functional activity in the glands themselves, then we could set out the factor for a particular degree of functional activity, and the actual functional activity itself, thus :—

AA......................(AAAAAA).

If the factor for functional activity underwent any change, this change would be shown in the functional activity itself, thus :—

Aa ....(AaAaAa).

If a still further change were effected in the genetical factor, this, too, would be seen in the character itself, thus :—

aa ....(aaaaaa).

Therefore any inborn or genetical change in the germ-cells of the parents must of necessity be transmitted to the offspring, and this change is shown in the parts themselves, *which arise out of that part of the inheritance that has varied.*

BASIS FOR SELECTION.

From what we have said it will readily be seen that only genetical variations provide any basis for selection, since these only have any chance of being inherited. No matter how highly fecund the bird may be, if its high fecundity be due wholly or in part to an acquired condition, that high fecundity will not be hereditarily transmitted, hence its selective value is *nil.* On the other hand, an inborn or genetical variation, provided it be sufficiently definite, is transmitted, and for that reason its selective value is high, varying only, as we shall show in its proper place, by its degree of definiteness.

Theoretically it is easy enough to draw a distinction between genetical high fecundity, which is inheritable, and acquired high fecundity, which is not. But in practice it is not so easy, although it is infinitely more important. We can, however, do a great deal by eliminating all condiments and all forcing foods and by feeding the birds on as plain and simple a diet as possible. By this plan, if the birds be properly housed, managed, of the right age, and thoroughly healthy, the fecund variations may with some degree of safety be classed as genetical and therefore hereditary.

From experimental testing it has been found that by using either of the two following mashes with a little oats in the morning, the birds are not "forced" on the one hand, nor "retarded" on the other, so that their records provide a sound basis for selection.

DRY MASH.

Middlings	2 parts by measure.
Ground Oats	2 parts by measure.
Pea-meal	1 part by measure.
Fish-meal	1 part by measure.
Clover Meal	3 parts by measure.
Bran	28 parts by measure.

This is kept continually before the birds.

WET MASH.

Boiled Cabbage	6 parts by weight.
Fish-meal	1 part by weight.
Middlings	8 parts by weight.

This is fed once a day and preferably at night.

Neither of these methods of feeding has the slightest stimulating effect upon the reproductive organs, but on the other hand they keep the birds sufficiently well nourished to avoid any risk of them re-absorbing the yolk. It should be distinctly stated that either of these mashes in conjunction with the oats *does not constitute what is known as a "Laying Ration," the object being purely to supply a sufficiently nourishing diet to maintain the birds in perfect health, while having no power to affect the genetical factor, thus allowing this to show a true development, unaffected by modifying influences.*

RECORDS AND AVERAGES.

There is only one method by which the fecundity of a fowl can be determined, and that is by counting the number of eggs she lays. There are three ways in which this is commonly done. Let us see what they are.

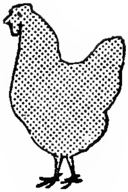
Flock Averages.—The aggregate of eggs laid by the entire flock in a single year is divided by the number of birds comprising the flock, and the quotient represents the *flock average*. This system is repudiated by most scientific breeders on the ground that it does not show the value of individual birds for stock, quite overlooking the fact that it was never intended that it should. The flock average, if taken every year, shows whether the general fecundity of the stock is satisfactory or the reverse, it shows what progress (if any) the stock is making, while it frequently indicates where money is being made or lost. The individual record (most valuable though it be) shows none of these things, for the individual records, except in very small flocks, have no bearing on the average of the flock. Moreover, the fact of the highest yearly record showing an annual increase is not an indication that the annual flock average is increasing ; quite frequently it is the reverse. And since the success of the strain must be based on the flock, and not on the individual, this taking of the flock average is a vital yearly necessity.

Pen Averages.—This is a kind of dissection of the flock averages. Its value consists chiefly in determining (1) the most productive breeds, strains, and groups of the flock, (2) the productivity of the offspring of certain matings, (3) the relative values of (1) and (2). Most especially is the pen average of service in assisting us to eliminate those *parts* of the flock which are below the general standard. The pen average, like the flock average, is deserving of more attention from scientific breeders.

Individual Records.—These are obtained by trapnesting each bird for a whole year, and show the individual variability. Their value, unlike flock and pen averages, is specific in so far that it is purely selective, and is almost the only means whereby we are able to determine that one bird is more highly fecund than is another The individual records have three points of distinct interest and value. These are : (1) the fecundation for the entire pullet year, (2) the fecundation during the winter months, and (3) the fecundity cycle.

Plate 4.

PARENTS.



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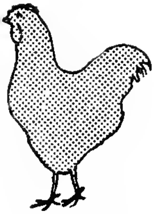

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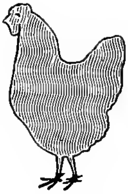

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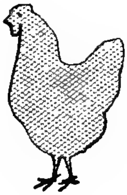

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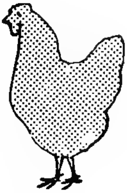

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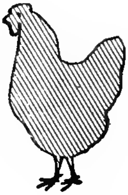


No. 16.

Showing that when two 10-egg fluctuants are mated together their female offspring fluctuate between 1 and 30 eggs for the winter test.

To face page 15.

The cycle will be dealt with later. The period covered by the winter months is usually intended to mean a period of thirteen weeks, starting either on the 15th of October or on the 1st of November—that is, from October 15th to January 15th inclusive, or from November 1st to February 1st inclusive. The fecundation which occurs during these winter months has a very much higher selective value than has the yearly record, because it is less likely to be increased by favourable weather conditions, the effects of which are not, of course, inherited.

FLUCTUATING VARIANTS. (*See Plate* 4.)

Now when we come to consider the individual records of fowls, we find for every flock that there are three classes of layers: those which are good, those which are moderate, and those which are poor. The " moderates " comprise by far the largest class, the number of " moderates " being usually as many as the " goods " and " bads " put together. The second largest class is variable; in some cases the good layers are far more numerous than are the bad layers, in other cases the two are about equal; in still others the poor are in excess of the good; while in all cases the rule appears to hold good that the moderates are by far the largest class.

The lines of division separating these three groups are well defined, the three classes themselves providing an example of what is meant by dimorphic variations. But even after we have divided the fecund types into their three respective classes, we still find that there is a large amount of variation *within the class*, and it is these small, ill-defined variations that we know as " fluctuating."

Let us more clearly define the three classes, which we will do in respect to their winter egg production. We will call them L2, L1, and Zero respectively, and classify thus:—

L2.—Birds giving over 30 eggs during three winter months.

L1.—Birds giving 30 eggs or under during three winter months.

Zero.—Birds giving no eggs during three winter months.

Taking the L1 class, it is clear that you may have quite a range of variants which still all conform to the L1 character, for during the three winter months the bird may lay any number from 1 to 30 eggs, and still be an L1 bird. These small fluctuating variations *are not inherited,* and this is a point which must be made very clear. The essential facts are :—

1. The L1 factor is inherited, which is proved by the fact of two L1 fowls giving only L1 pullets.

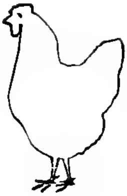
2. The fluctuant is *not* inherited, which is proved by an L1 hen with a winter record of, say, 10 eggs, being mated to a male whose dam also had a winter record of 10 eggs, when it will be found that their female progeny fluctuate between 1 and 30 eggs for the winter record, whereas they should lay exactly 10 eggs each were the fluctuant inherited. (See Plate 4.)

According to Pearson, by a long and painful process of selective breeding it is possible to make a fluctuant breed relatively true to the extreme range of its variability—but not beyond. This means that if generation after generation we mated hens with 30-egg winter records to males bred from dams with 30-egg winter records, eventually the vast majority of the female progeny would lay exactly 30 eggs during the three winter months. As Pearson estimates that it would take some hundreds of generations to accomplish this, he is in a particularly comfortable position, as during his lifetime, at any rate, it is not possible to experimentally prove that he is wrong.

Whether Pearson be right or wrong in his assumption, the fact remains *that for all practical purposes fluctuations are not inherited,* and the sooner we recognise this simple truth the sooner shall we cease to pay exorbitant prices for a dozen extra eggs on the year's record,

Plate 5.

PARENTS.



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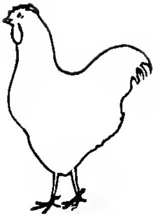

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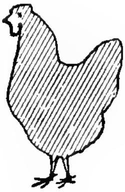

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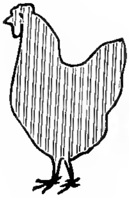

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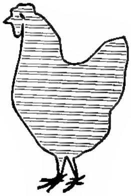

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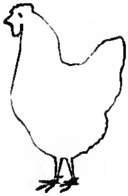

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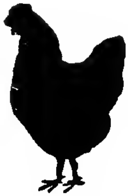


No. 2.

Showing a dimorphic fecund variant (No. 2) from a pair of Zero birds.

To face page 17.

DIMORPHIC VARIATIONS. (*See Plate* 5.)

As we have already pointed out, the L2, L1, and Zero types present examples of dimorphic fecund variations, and by reference to Plate 5 we shall see that such variations, unlike the fluctuating kind, are highly inheritable. This is because they are definite, and are not merely examples of degree in factorial equilibrium, as are all fluctuants. The dimorphic variation is sometimes called a " sport " and sometimes a " mutation." Actually it shows definite discontinuity between one type and another.

So far as discontinuity in variation be concerned, there is no real difference between a dimorphic and a reversionary variation (since both are discontinuous), but it is often necessary, in order to present facts in a concrete and understandable form, and also for convenience in classification, to make some kind of distinction where actually none exists. For this reason we shall define a dimorphic variation as " a definite variation of an evolutionary nature," while a reversion we shall style as " a definite variation of a retrograde nature." Both, as we shall see, are inheritable.

Now, although dimorphic types breed true, this must not be understood to mean that they never give rise to other and further dimorphic variations, because the whole history of organic evolution shows that they do. Nor do I think that any experienced breeder would even think of selling a pen of birds with a written guarantee that they would never " sport."

Dimorphic variations are not necessarily extreme variations, but are simply, as pointed out, *definite* ones. Nevertheless, they are perhaps best illustrated when taken in their most extreme form. If, for example, we take a very low fecund Zero hen and mate her to an equally low fecund Zero male, the only thing we can *calculate* upon is that all the progeny, like themselves, will be Zero—that is, capable of laying no eggs during the winter test ; the *yearly* records of their progeny will, however, vary, as shown on

Plate 5. The fecund variants, Nos. 18, 20, 22, and 24, we can depend upon obtaining from *every* mating of Zero birds. These are mere fluctuations, possessing no selective value.

When, as sometimes happens (the phenomenon is not common), we breed a new fecund type (see Pullet No. 2, Plate 5), we have a case of dimorphic variation, and this is always liable to occur from any *inter se* mating of pure types. These dimorphic variations are of extreme value, *because they always breed true.* This does not mean that they breed true to the fluctuating variation, but to the new fecund *type.* This can be set out more explicitly. The dimorphic variation in this case is an L2 pullet from two Zero parents. This bird, if correctly mated, *will breed true to the L2 factor.* If we further examine this bird, we find that its winter egg production is over 80. The range of fluctuations for the L2 type is from 31 winter eggs upwards. Therefore 80 represents a fluctuation, and not a type, and the bird, although giving L2 offspring if correctly mated, will certainly not breed true to the 80 eggs.

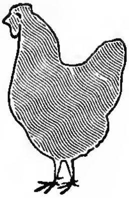
REVERSION TO TYPE. (*See Plate* 6.)

We now have to consider a very common phenomenon which most frequently occurs when we breed together two highly fecund birds of two totally unrelated strains. As we know, even our highest fecund birds have in the first place been evolved from very low fecund ancestors. These ancestors may be very remote, but nevertheless they exist and are represented in the germ-cells of their descendants. It is not only necessary that we should admit this point, but it is equally necessary that we should understand it.

To some extent at least evolution has been brought about by new combinations of germ-cells. This must occur every time that fertilisation takes place in sexual organisms, so that each new organism born of sexual union must contain factors from *both* its parents. Each of these parents in its turn also contains factors from both *its* parents, and

Plate 6.

PARENTS.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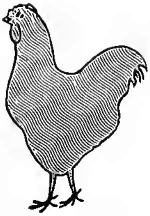
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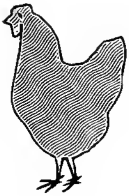
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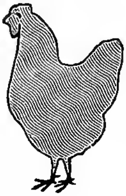
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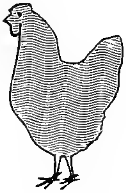
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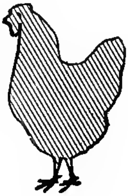
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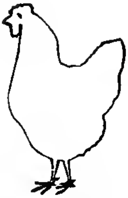
No. 24.

Showing two reversionary types (Nos. 16 and 24) arising when two L2 birds of unrelated strains are mated together.

To face page 18.

so on back through an infinity of time. Thomson very aptly describes each living organism as a vehicle through which is passed to its descendants much of what it has itself inherited. If, as we know, both of the parents do this, then it is not hard to understand that by fortuitous circumstances a re-combination of factors may be brought about in some of the offspring of a given mating which more closely resembles an ancestral type than it resembles either of the parent types. This would result in *Reversion to Type.*

Highly fecund types, for example, have been evolved by one or more dimorphic variations (see Plate 5) from very low fecund types, so that they are capable, if circumstances be favourable, of throwing offspring which it will be found have completely reverted to one of these earlier ancestral types. If, as is probable, the L2 bird was evolved from L1, which in its turn was evolved from Zero, then the L2 bird will be found capable of giving offspring reverting to either of these two types. If, however, as shown on Plate 5, the L2 type has arisen as a dimorphic variation from Zero (instead of Zero giving L1 and then L1 giving L2), then it can only revert to Zero, as under those circumstances no L1 bird would have figured in the ancestry.

The crossing of strains or of breeds is the most frequent cause of reversionary types (see Plate 6), for when we breed from totally unrelated birds we encourage a re-combination of old groups of germ-cells which, often for generations, have remained separated in the course of descent. We may, and sometimes do, bring together such re-combinations when the parent birds are closely related to each other; but the chances are against it because related birds are far more likely, through a more common ancestry, to inherit the same kind of germ-cells; hence their progeny tends to be more like themselves.

TESTING FEMALES.

Before attempting to mate birds to produce highly fecund offspring, it is most essential that we should test them in order to ascertain whether they be L2, L1, or Zero types. This, in respect to the females, is done by trapnesting, for, although, as will be shown later, there are several exterior characters which indicate to which fecund type the bird belongs, still these are frequently deceptive, and the trapnest record is at all times the most reliable.

Before dealing with the actual work of trapnesting, we want to bear in mind that there is no object in testing all birds bred, for in every flock of pullets there is quite a number that can be eliminated as stock just as easily before as after the test. These are those pullets which by mid-October or early November are (1) immature, (2) in moult, (3) delicate, (4) malformed, (5) very coarse in the bone, (6) very large or very small for the breed, or (7) of poor fecund ancestry. Birds coming under (1) or (2) categories may be subjected to the test for what is known as the (L2) factor, which will be considered at the end of this volume. Birds coming under the categories (3), (4), (5), (6), or (7) need not be tested at all, as they are worthless, and should be eliminated from the flock as speedily as possible.

There are several points to be remembered in the actual work of testing by means of the trapnest, the neglect of which is to very seriously stultify the work itself.

1. The birds should be placed in the house at least three weeks before the test is due to start.

2. Each bird should be rung with a numbered or coloured leg band.

3. Each bird must be red in the comb and have its pelvis bones wide apart when placed in the pen.

4. One trapnest should be allowed every two birds, and a reliable trap-nest should be used. The trapnest should be as near silent as possible, absolutely dark when the shutter is down, incapable of allowing two birds to enter it at the same time, one in which the " release " will act every time a bird enters it, that will not allow a bird to come out until she is released by the attendant, one which will not allow another bird to enter when it is already in service, and one which is easily kept in a sanitary condition.

5. The birds should not be kept Intensively, and should only be confined to the house during very bad weather.

6. Eggs not laid in the traps should not be recorded.

7. Very small eggs, rough shelled eggs, shell-less eggs, and mis-shapen eggs, should all be recorded, as the object of the winter test is to record the number of ova passed in a given period ; a separate note can be made of irregularities in shape, size, and shell.

8. A double-yolk egg should be recorded as two eggs since two ova have been passed. A very tiny egg containing only albumen and no yolk, as is occasionally laid, should not be recorded, as no ovum has been passed.

9. The birds should be fed as advised under *The Basis for Selection.*

10. The eggs should be collected several times a day, and especially during the morning should the traps receive constant attention.

If all of these points be carefully observed, there are two kinds of winter records which will justify us in selecting the pullet for stock. These are :—

(*a*) Over 30 eggs from October 15th to January 15th, or

(*b*) Over 40 eggs from November 1st to February 1st.

These are the L2 birds. Any pullet failing to pass either of these tests should not be used for stock, the only exception being in the case of L(2) birds, which are dealt with at the end of this book. When building up a highly fecund strain of birds, it is most essential that each generation of pullets be tested for the L2 factor, and that these alone be included in the breeding pens.

TESTING MALES.

Hitherto we have only referred to the females as coming under the categories L2, L1, and Zero, but we must now consider how the males stand in respect to these three factors. It is, of course, self-evident that if these factors be carried at all by the males it must necessarily be in a latent condition, for although various degrees of fecundity may be represented in the germ-cells of the male, they are not developed by him, although there is clear evidence that he can transmit various degrees of fecundity to his female offspring, and that this will have a most marked influence on their egg production.

Dr. Raymond Pearl has suggested that the dam transmits her fecund factor to her sons and that they, in their turn, transmit it to their daughters, which in due course develop it. While admitting the value of Pearl's work and its correctness in some respects, our own research work goes to show that in detail it will have to be considerably modified. Pearl's contention was :—

(1) That an L2 male was one bred from an L2 hen.
(2) That an L1 male was one bred from an L1 hen.
(3) That a Zero male was one bred from a Zero hen.

The points in which we differ from Pearl can best be shown by the following statement, which our experience from the matings of various fecund types suggests to us as correct :—

(*a*) L2 hens give both L2 and L1 males.
(*b*) L1 hens give both L1 and Zero males.
(*c*) Zero hens give only Zero males.
(*d*) Exceptions may occur either in the case of dimorphic or reversionary types, as we have already explained, but these variations are the exception and not the rule.

According to our contention, then (and it is supported by a great deal of evidence collected from our own research work and from that of other experimentalists), it does not in the least follow that because a male is bred from an L2 hen that he himself is L2, and therefore able to transmit L2 to his daughters. He is frequently L1, and our experience is that L1 males will give mostly L1 daughters, and sometimes some Zeros. This is a very serious matter, and to remain in ignorance of it, or not to appreciate it at its true value, is to run a grave risk of breeding a very high percentage of low fecund pullets, if not to ruin the fecundity of the entire stock.

It is therefore most essential that males from L2 hens should be tested in order to determine whether they be L2 or L1, *and only the proved L2 males should be used for stock.*

The method of testing males is a very simple one, and may be explained thus :—

(1) Take as many males as possible from a proved L2 hen (L2 hens are Nos. 2, 4, 6, and 8, on Plate 1), and (2) mate these to proved L2 pullets, making up as many pens as possible, even if you are only able to put a couple of pullets with each cockerel, so as to test as many males as possible. (3) Trapnest the pullets bred from each of these matings. (4) Those males breeding as shown on Plate 7 are L2 males, and (5) those males breeding as shown on Plate 8 are L1 males.

When the L2 males are discovered, these can be run with a pen of 7 or 8 L2 hens, when all the female progeny will be as shown on Plate 7. The L1 males should be disposed of.

Objections may be raised to this plan on the score that it necessitates breeding with males in their second season. That is not a valid objection ; first, because males in their second season (there are some exceptions, of course), are usually as fertile as when in their first season, provided that proper care be taken of them ; and secondly, because the *quality* of the pullets is of far greater importance than is the *quantity*. From a purely financial point of view, it will pay to breed only with proved L2 males mated to proved L2 females, so long as the birds will breed any chickens at all.

To sum up, the testing of males is most essential if high class fecund stock is to be bred in any quantity. The testing of males by breeders is a matter claiming urgent attention, and I am fully convinced that no real or permanent progress will be made by owners of pedigree fecund stock until such time as this practice is one of wide and general adoption.

TRANSMISSION OF THE FECUND FACTOR.

In studying the transmission of the fecund factor with a view to breeding a highly productive strain of fowls, it is necessary that we should consider not only how the L2 factor is transmitted, but also the manner in which L1 and Zero are inherited. In other words, it is just as essential to know how *not* to breed bad layers as it is to know how to breed good ones. Much valuable information is to be gained by cross-breeding the various fecund types, as well as from the results of the various *inter se* matings. For that purpose we propose giving every possible definite mating, around which all others will be found to group.

Before considering the actual transmission of fecundity, it should first be pointed out that there are four conditions which may make the results obtained by breeders at variance with the results shown in the following plates. These are :—

(1) The fecund factor carried by the male may not be the same as set out in the diagram. This is likely to occur where an untested male is used, when the fecund factor he carries can only be assumed and not known. (*See Testing Males*).

(2) The fecund factor carried by the female may not be the same as set out in the diagram, owing to her fecundity being "acquired" and not "genetical." (See *Acquired Fecundity*).

(3) Dimorphic variations are in some cases liable to occur. (See *Dimorphic Variations*).

(4) Reversionary types are also sometimes liable to occur. (See *Reversionary Types*).

Both (1) and (2) may be avoided, but (3) and (4) are beyond the control of the breeder.

In those diagrams showing that more than one fecund type occurs from a given mating no attempt has been made to show the ratio in which these types occur. Since these are highly variable from different matings, even where the same fecund factors are involved, it would be the height of folly to represent them as being constant, or even to

Plate. 7.

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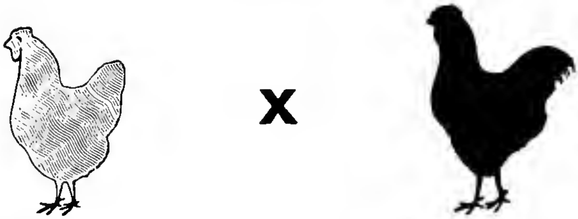
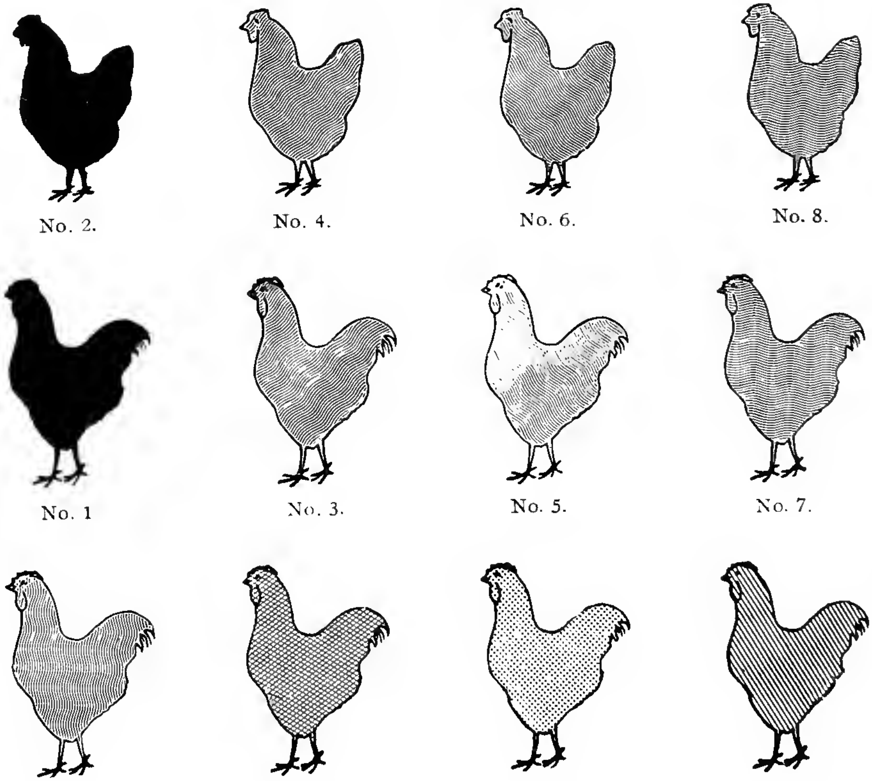


No. 6.

No. 1.

PROGENY.



No. 2.
No. 4.
No. 6.
No. 8.

No. 1
No. 3.
No. 5.
No. 7.

No. 9.
No. 11.
No. 13.
No. 15.

Showing the female and male fecund types bred from two L2 birds.

To face page 25.

venture upon an example, as none would be typical. Therefore types but not ratios are shown. Plate 1 shows fluctuations in L2 types ; Plate 2 shows fluctuations in L1 types ; and Plate 3 shows fluctuations in Zero types.*

L2 × L2. (*See Plate* 7.)

This is, of course, the ideal mating. It is the one which every intelligent breeder tries to make, but one in which he more often fails than succeeds. The reason for this failure is plainly shown if we study Plate 7.

From two genuine L2 birds only L2 females are bred. The individual pullets from this mating may, and in point of fact, do, show every conceivable variation within the limits of their type ; but they do not go beyond the type (unless a rare reversionary type be bred), and are therefore all actually L2 birds. That shows the extreme value of this mating : it gives only excellent winter layers.

But although the females are all of one type, the male progeny show two types—both L2 and L1 cockerels being bred, and it is absolutely impossible, *without testing in the manner already described*, to distinguish one from the other.

It is contended that the highly fecund factor is transmitted by the dam to her sons and by the sire to his daughters. Let us examine this statement in the light of the facts illustrated on Plate 7.

The male (No. 1) is bred from a dam with a winter record of over 80 eggs and a yearly record of over 280. This bird should, according to the common contention, give us only L2 pullets if mated to an L2 hen. He is mated to hen No. 6, which is an L2 type giving between 40 and 49 eggs for the winter test, and with a yearly record of between 200 and 229. The female offspring show winter records of from 31 upwards, and yearly records of 140 upwards. The common contention is therefore correct in respect to the pullets bred from such a mating.

---

*These three plates should be consulted when studying the following matings, as they provide a key to the various shadings.

The next point of common contention is that an L2 hen breeds only L2 males when bred to an L2 cock. By reference to Plate 7, we find that this is not the case, as both L2 and L1 males appear from the mating. The reason for this is not yet clear, although it is probably due to the facts that (1) the L2 factor can only be transmitted to the males by their dam, and that no L2 hen is able to carry the L2 factor in a " pure " state. Be this as it may, the fact remains that experimental breeding shows that both L2 and L1 males are bred from two L2 parents ; hence the necessity for testing all males.

Nevertheless, this does not do away with the fact that *an L2 male can only be bred from an L2 hen.* L2 males are bound to occur from such a mating : what we have to do is to discover which they are.

L2 × L1. (*See Plate* 8.)

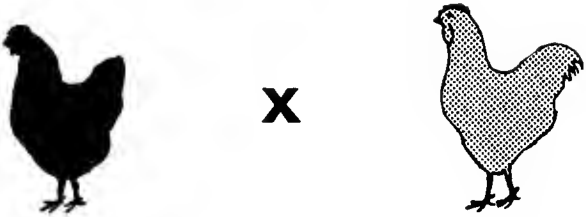
This mating is more frequently made through carelessness than through choice, and how expensive such carelessness is we are now about to show.

As we have already explained, it is generally *assumed* that all males bred from an L2 hen are themselves L2, so that any one of them is equal in value to any one of the others for stock. We have seen that this is not so. But it will readily be seen that working on this entirely false assumption one may easily (under the impression that he is an L2 bird) select an L1 male for stock, and mate him with genuine L2 hens. Plate 8 illustrates what will then happen.

If we select, say, male No. **13** on Plate 7, under the impression that he is like No. 5, all of the female progeny is L1 and practically worthless. This is so even when, as on Plate 8, he is mated to genuine L2 hens of proved high fecundity. Now, had this male been either No. **1**, **3**, **5**, or 7, from the same parents, and had he been mated to L2 hens, he would have given the same class of pullets as shown

Plate. 8.

PARENTS,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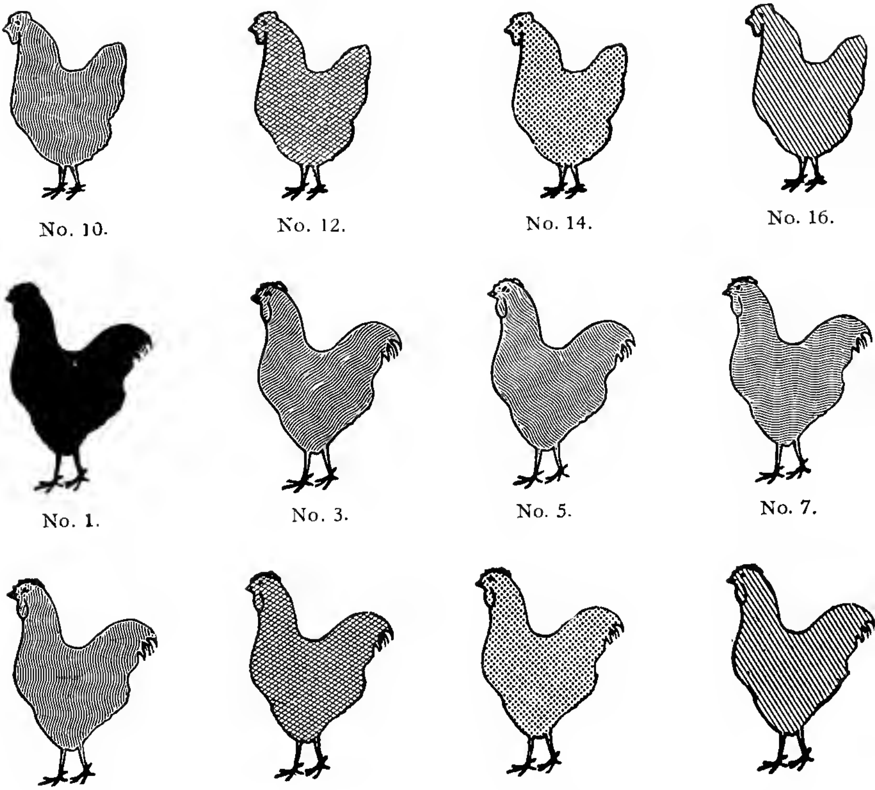

No. 10. No. 12. No. 14. No. 16.

No. 1. No. 3. No. 5. No. 7.

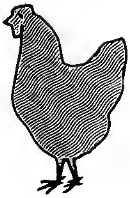
No. 9. No. 11. No. 13. No. 15.

Showing the male and female types bred when an L1 cock is mated to an L2 hen.

To face page 26.

Plate 9.

PARENTS.



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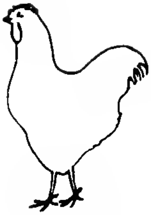

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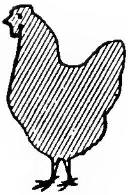

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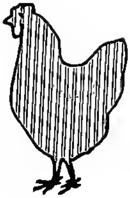

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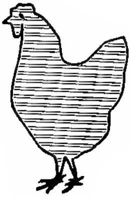

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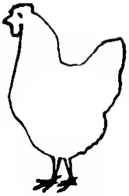

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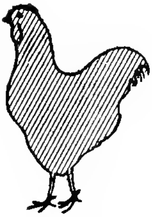

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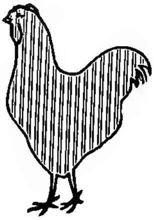

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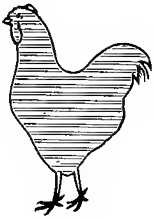

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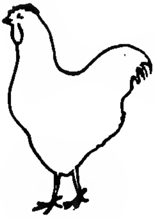

No. 23.

Showing the complete dominance of Zero over L2.

To face back of Plate 8.

on Plate 7. Thus, the failure to test males from highly fecund hens frequently results in season after season being wasted.

This mating, however, illustrates something more than the importance of testing the males—it shows, if studied in conjunction with Plate 7, exactly how the L2 and L1 factors are inherited.

Taking the males first. We have practically the same range of fecund types from L2 × L1 as from L2 × L2. The significance of this lies in the fact that it shows from which parent the males inherit the fecund factor they carry, namely, their dams. In both matings is the dam an L2 bird ; in the first mating the sire is also an L2, in the second he is an L1, but from both matings there are both L2 and L1 males. If we had only the mating set out on Plate 8 to judge by, we should naturally have assumed that the L1 male was accountable for a proportion of L1 male progeny, but when we find that these L1 males still occur when the sire is L2, we cannot reasonably submit that the sire has had any influence on the results. Thus we see what is meant by the males inheriting L2 from their dams.

Turning now to the females, we find that the substitution of an L1 for an L2 male (the female still being L2) has resulted in all the female progeny being L1 instead of L2, as in the previous mating. Thus we observe that while a change of sire *does not* influence the fecund factor carried by the male offspring, it totally alters the fecund factor developed by the female offspring. This shows what is meant by the pullets inheriting L2 or L1 from their sire.

L2 × ZERO. (*See Plate* 9.)

We come now to a very common phenomenon—that known as simple and complete dominancy. So far we have seen the fecund factor of the pullets inherited from their sire and the fecund factor of the cockerels inherited from their dam. We now have to consider a case in which this sexual inheritance does not hold good—a case in which *all* the progeny, irrespective of sex, inherit their fecund factor from only *one* parent.

When a Zero male is mated to L2 hens, all the progeny is Zero. (See Plate 9). The question arises as to how this result is brought about. Why should we have sex-heredity when L2 hens are mated to either L2 or L1 males, while when the same hens are mated to Zero males there is simple and complete dominance of the Zero factor ?

No doubt the explanation is to be found in the fact that while Zero is the normal state of fecundity in fowls, both L2 and L1 are largely artificial in so far that they have been brought about by man's selective breeding. Let us examine the facts more closely in order to ascertain whether this is a probable explanation.

In all domestic animals and plants we have a number of new varieties which have sprung from wild aboriginal forms. So far as we are able to judge, these breed true to type *so long as they be bred among themselves*, but no sooner are they crossed with the aboriginal wild type than *all* the progeny is like the latter. Further, if any of these new domestic varieties be turned loose and allowed to breed promiscuously, they speedily revert to the wild type. Only one thing can be deduced from the facts, and that is this : that new varieties, types, or characters evolved by selective breeding are constant (*i.e.*, breed true) only so long as the selective breeding is continued ; when this ceases they, at first slowly and then more rapidly, revert to the aboriginal or " normal " type. Still further, we find that the greater the fluctuation from the mean the less constant is the inheritance of degree. Thus two birds carrying an 80-egg winter factor will not breed relatively so true as will two birds carrying a 31-egg winter factor, although all are L2. Therefore the extreme of a fluctuating variant requires the *most intensive selection* in order to keep it anything like constant.

Plate 10.

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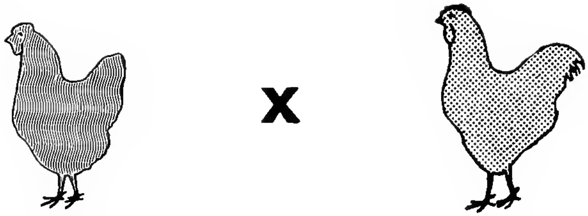
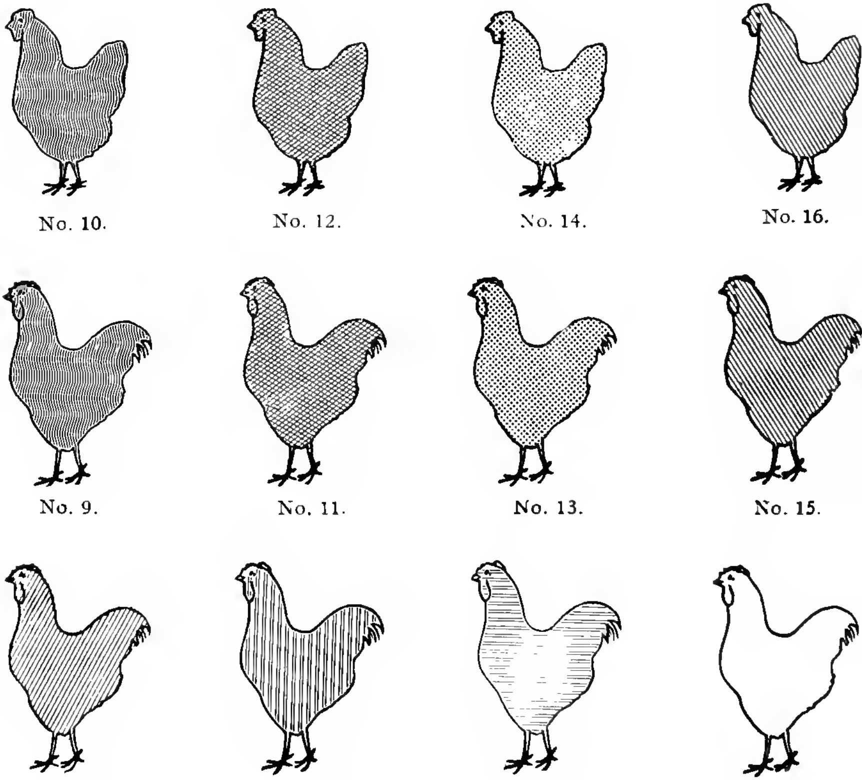


No. 10. No. 13.

PROGENY.



No. 10. No. 12. No. 14. No. 16.

No. 9. No. 11. No. 13. No. 15.

No. 17. No. 19. No. 21. No. 23.

Showing the female and male types bred from two L1 birds.

To face page 29.

The wild aboriginal fowl *does not* lay during the winter months ; hence all are Zero, and Zero fecundity must be taken as the normal of the species. L2 and L1 are aberrant types, and when they are crossed with Zero or normal type, they are simply " swamped." As will be seen from Plate 9, the L2 mother has absolutely no influence on any of her offspring when fertilised by a Zero male.

L1 × L1. (*See Plate* 10).

Having considered how the L2 hen breeds when mated to L2, L1, and Zero males, we must now consider the inheritance of fecundity when L1 is introduced from the female side. For this purpose we will first take an *inter se* mating between two L1 birds, as shown on Plate 10.

It will be observed in the first instance that again, as when breeding with two L2 birds, *all* the female offspring conform to type—that is to say, that they are all L1, just like the two parents and, as we shall show more clearly in our next mating, they inherit this factor from their *sire*. Neither L2 nor Zero female types appear from this mating, so that it is a perfectly useless one if we wish to breed winter layers, as the winter production of these pullets varies from one to 30 ; but does not exceed 30 unless a dimorphic L2 type occurs, which is not a thing we could count upon.

According to current opinion, all the males from this mating should be L1, but they are not—many Zero males occurring. For merely scientific purposes, or to confirm these remarks, it is worth testing these males out, but no *practical* end would be served by doing so, as they would all be found quite worthless for stock so far as breeding for egg production is concerned.

Now the fact of both L2 and L1 males being bred from an *inter se* mating of L2, in conjunction with the fact that L1 and Zero birds are bred from an *inter se* mating of L1, throws an extremely interesting light upon the inheritance of fecundity—it discloses certain facts which the breeder will do well to observe. These facts are :—

(1) That there is a decided tendency for each of these fecund types to revert to poorer types, L2 reverting to L1 and L1 reverting to Zero.

(2) That these reversions first take place in the males, and through them are transmitted to the females.

The point of paramount importance arising out of these two facts is the urgent necessity for the most rigorous selective breeding. If highly fecund stocks are to be maintained —and we know how they deteriorate in the hands of some breeders—then every bird, both male and female, must be rigorously tested for the fecund factor it carries, and only the very finest selected for stock. And this selection must be systematised throughout: it is of no use to test all breeding stock of both sexes one year and to omit to do this the next. Such slipshod methods of selection and breeding will never result in a uniform high flock average. It cannot. Reversionary tendencies of the kind we are here considering are permanent in each succeeding generation, so that only the most constant and rigorous selection can prevent them from exerting an evil influence.

L1 × L2. (*See Plate* 11).

In many respects this is a most remarkable mating, showing not only the widest possible range of fecund types, but also providing us with much very valuable information respecting the exact genetical position of the types themselves; it is, in fact, a mating which, above all others, has provided us with most information as to how fecundity is inherited.

As will at once be seen, the L1 hen again gives L1 and Zero males, just as she did when mated to a male of her own type, the substitution of an L2 bird having no power to alter the fecund constitution of her sons. This is merely pointed out as providing fresh evidence, if more were needed than that already given, that the female transmits her fecund *late 11.*

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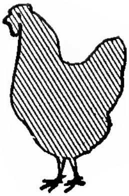

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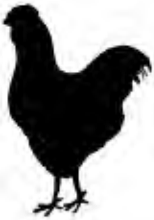

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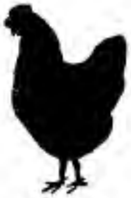

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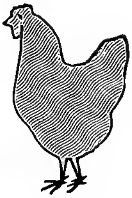

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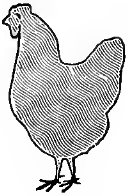

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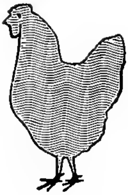

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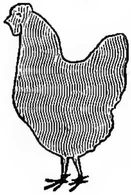

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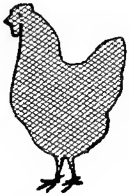

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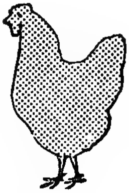

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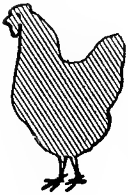

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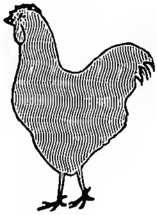

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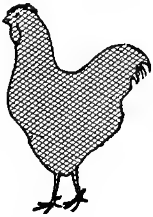

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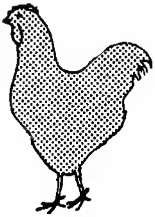

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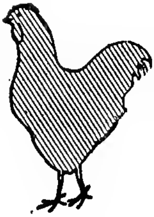

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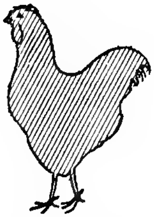

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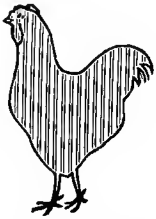

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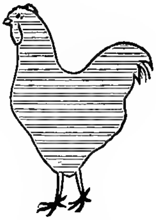

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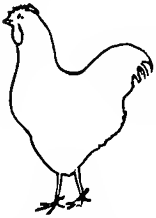

No. 23.

Showing the range of female and male fecund types when an L1 hen is mated to an L2 male.

To face page 30.

factor to her sons , the fact that some of her male offspring carry a *lower* fecund factor than that possessed by herself in no way invalidates this statement.

When we turn to the *female* offspring of this mating, we find ourselves confronted by a new phenomenon in the inheritance of fecundity, and one which yet has to be considered.

We have hitherto seen that *all* the females resemble their sire in respect to the fecund factor ; but here is an exception, for in this mating we find that while some of the pullets show their sire's type, the remainder are L1 like their dam. How are we to account for so unexpected a result ?

I think the study of Mendelism will probably provide us with the correct solution. According to the Mendelian principle, the type may be either a complete dominant, an irregular dominant, an imperfect dominant, a recessive, or it may be latent. Its inheritance can also be influenced by sex. We need only consider two of these conditions.

(1) If the type is behaving as a complete dominant, it may be either homozygous or heterozygous. If it be homozygous, *all* the progeny will be like itself if mated to a recessive ; if it be heterozygous, only half the progeny will be like itself when mated to a recessive.

(2) If the type is behaving as an irregular dominant, it must be heterozygous, but when bred to a recessive, it will give a majority of offspring like itself, and will differ from a heterozygous complete dominant in that the ratios will vary and cannot be worked out, as can the latter.

What evidence we have at our disposal points very strongly to the L1 factor behaving as an irregular dominant when fertilised by L2, as considerably more than one-half of the total progeny (that is including both male and female) are L1, and the number of L1 birds is certainly most variable from different individual matings of L1 × L2.

Two other points must be not overlooked. The irregular dominance of L1, when L1 is fertilised by L2, would in no way account for those L2 birds which are bred always being females. This is due to quite another thing, namely, to a degree of sexual inheritance that still persists which, as we have already seen, is such a marked feature in the transmission of the fecund factor.

The other point is the Zero males. The irregular dominance is not accountable for these. As we have already seen, and as we shall show still more clearly in other matings, which will come up for consideration later, there is a constant tendency (but more especially in the males) to revert to a lower fecund type than are either of the parents, provided that either or both of the parents are L2 or L1, and this tendency is in no way attributable to the fecund genetical constitution of either parent or of both.

L1 × ZERO. (*See Plate* 12).

The offspring of this mating is in every way similar to that obtained from L2 × Zero, namely, all the progeny is Zero. There are, however, one or two points to be considered here which could not very well have been treated earlier.

We have already said that the Zero is the normal type for fowls; that is to say, that it is "more natural" for birds to lay no eggs during the winter months than it is for them to lay even a very few. But if it is not natural for fowls to lay during the winter months, neither is it "natural" for them to put up, say, even 60 eggs in a year, and many Zero hens will lay considerably more than that. Thus, we often have the phenomenon of a hen being normal (Zero) for the winter months, but above the normal (and sometimes considerably above it) for the spring and summer months.

It is perfectly true that none of the females shown on Plates 9 and 12 are even passable layers in any sense of the word, but, nevertheless, anything above 30 eggs for the year must be regarded as an advance on the wild Jungle Fowl, and it is a point of interest to consider to what this "advance" is due.

Plate 12.

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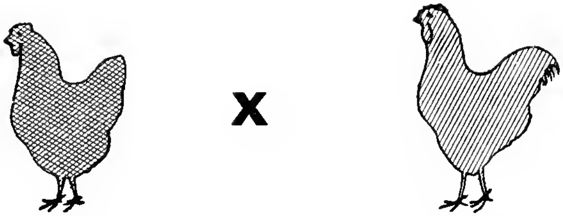

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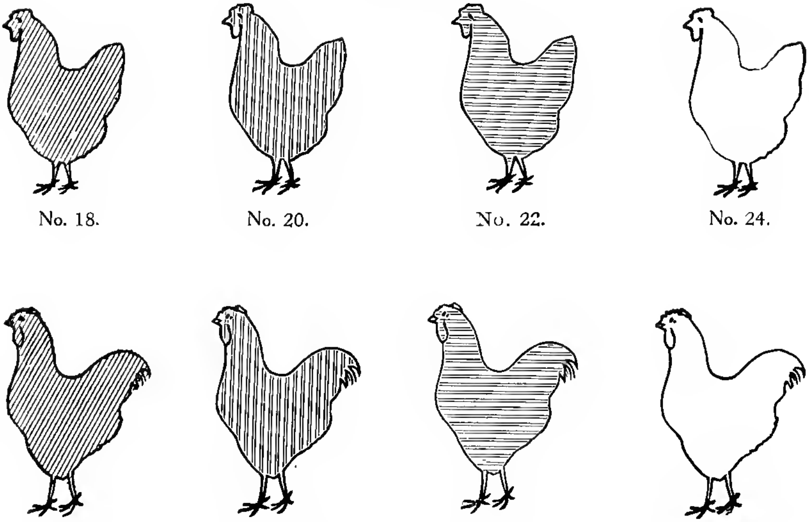

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No. 17. No. 19. No. 21. No. 23.

Showing the complete dominance of Zero over L1.

To face page 32.

Zero birds, as a rule (that is, taking the typical specimens for this class), are either mongrel or exhibition fowls which have been subjected to no selective breeding for egg production, so that the increase in the yearly egg-yield (however small it may be) over *Gallus Bankiva,* can hardly be put down to selection. Further, no better spring and summer layers are bred when a Zero male is mated to an L2 than when he is mated to an L1 hen, and this is all much against any spring and summer increase in Zero birds being due to a genetical variation.

The alternative is that this kind of increase is "acquired" Then there are three possibilities, and it is probable that all have played their part. (1) A greater quantity of food; (2) housing; (3) the eggs being collected daily and not allowed to accumulate in the nest. Of these, no doubt (3) has had the greater influence because it is the more direct. The natural habits of all birds, after having laid a certain number of eggs, is to incubate them, and the ovary is so adapted that, even with wild birds of all kinds, if the eggs be removed from the nest as laid, there will be a wonderful response in fecund activity on the part of the ovary, resulting in a largely increased number of eggs. By taking the eggs away as laid from pigeons, canaries, budgerigars, and other birds, and by not allowing them to be incubated, I have myself increased their production over 30 per cent.

If all eggs over 30 laid during the spring and summer months by Zero birds be merely an "acquired" condition, it would have no selective value, and a strain of heavy layers could not be evolved from such Zero types simply by mating together, even for a series of generations, the extreme fluctuations for spring and summer production.

It should be repeated that it is the Zero type which proves a simple dominant, *and not the yearly record.*

ZERO × ZERO. (*See Plate* 13).

This mating breeds absolutely true. That is to say, a hen giving no eggs in the winter, mated to a male bred from a hen also giving a *nil* winter record, would give only Zero daughters. This mating, while possessing no direct practical interest, yet gives rise to a point which is deserving of at least some attention.

The point is speculative. It is this: since the Zero type is the normal type for the species, since Zeros breed true when mated together, and are dominant when mated to other types, and since there must have been a time when nothing but Zero types existed, how have L1 and L2 types been evolved?

The Neo-Darwinian school would contend that these types were evolved by artificial selection, the highest fluctuating variants proving the basis for this selection. This can be put in a concrete form. A consideration of the progeny set out on Plate 13 shows that the extreme *range* of the fecund fluctuation in Zero types is a yearly record of 80 eggs at one extreme and of less than 30 at the other. Now, if female No. 18 (80 eggs for the year) were mated to male No. 17 (also representing 80 eggs for the year), according to the Darwinian theory a few—a very few—of the offspring should show a *very slightly* increased yearly record, and would lay, perhaps, just one or two eggs in the winter (these being L1 birds). If generation after generation those birds showing a very slight increase in fecundation over their fellows were mated together, eventually the L2 type would be evolved as a result of (1) continuous variation and (2) continuous selection of the best (highest) variants.

In our opinion such an hypothesis assumes too much. Let us see what it assumes:—

(1) It assumes that every generation the fluctuations will show a *progressive* tendency. That is to say, the extreme fluctuations in each generation will show at least a slightly higher egg record than did the extreme variations in the generation immediately preceding it. I submit that there is not a shred of evidence in support of continuous progressive fluctuation, but that on the contrary the range of variants in one generation is *exactly similar* to the range of variants in any other generation.

Plate 13.

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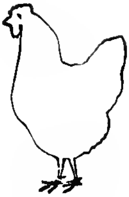
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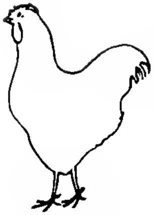
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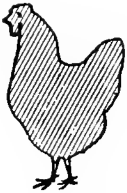
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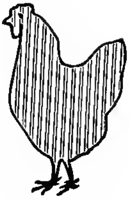
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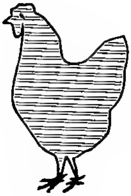
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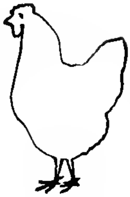
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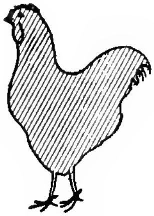
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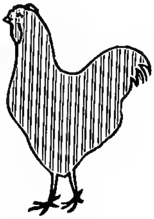
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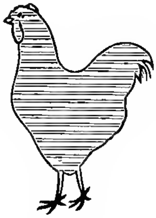
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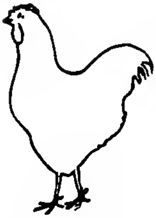
No. 23.

Showing that two Zero birds breed true to type.

To face page 34.

(2) It assumes that at least a *fair proportion* of the offspring in each generation will show this progressive variation, *as without a sufficient number of these progressive variations you obviously could not obtain the necessary matings.* If there be no evidence in support of assumption (1), there is still less in support of assumption (2), for if a progressive fecund tendency be not seen in *some* of the birds, it is quite certain that it could not be seen in anything like a fair proportion.

(3) Lastly it assumes the inheritance of fluctuating variants, of which there is not an atom of experimental evidence. On the contrary, start a man with a strain of badly marked Buff Orpingtons, and merely by mating together the *least badly marked* he will never produce a strain of Palace winners. Give him any average flock of Croad Langshans, and merely by mating together the *smallest* he will never evolve a strain of Bantams; give him a strain of Indian Game, and merely by using the *best layers* for stock, he will never produce birds fit to win a Laying Competition Now, all of these things would be quite possible were fluctuating variations inheritable, but we find that in spite of anything we can do, *we cannot by selective breeding alter any character so long as we have to confine our selection to fluctuating variants.*

On the whole, then, I do not think that the Neo-Darwinian school can tell us how L2 and L1 birds have been evolved; the theory assumes too much and proves too little.

The Neo-Lamarckian school would attempt to account for the facts in quite another manner. It would argue some what in this way:—(1) The organism is so constituted and is capable of such "responses" that it will rapidly adapt itself to changes in the environment. (2) When the wild Jungle Fowl was brought under the influence of domestication, the more abundant food, the better housing, especially at night, the change of climate when it was exported to other countries, and the human consumption of its eggs, called for "responses" in the ovary which resulted in an increased production. (3) These environmental influences working upon the individuals of countless generations at length penetrated to the germ-cells of the birds, when their effect came to be hereditarily transmitted.

This theory ignores several essential facts. Surely we are entitled to ask :—

(1) Why has the environment increased the fecundity of such breeds as the White Leghorn, while it has left that of such breeds as the Indian Game almost entirely unaffected ? The adaptation theory will not account for this ? but the difference is apparent to every poultry breeder.

(2) Allowing that the ovary and yolk-glands *do* respond to changes in the environment—and we have contended that they do under the heading of *Acquired Fecundity*—what evidence have we to prove that such adaptations would remain constant were the environment again changed ? If adaptations be inheritable, how is it that they only persist so long as the environment which gave rise to them remains constant, and are lost so soon as the birds are brought into the old environment again ?

(3) What evidence can be produced (either experimental or clinical) in support of the contention that adaptations are inheritable ?

Apparently the Lamarckian theory has no more to commend it than has the Darwinian theory. They are both speculative and entirely lack experimental proof.

The third view is that advanced by those who believe in the mutability of types. This view is that both the L2 and L1 types may have arisen as " mutations " or " sports." The general idea underlying this is (1) that at all times all types are liable to " mutate " or to " sport " new types ; (2) that such mutations or sports are due to changes in the protoplasm of the germ-cells ; (3) that these mutations may be either large of small, but that they are always definite ; (4) that mutations very seldom come singly, usually quite a number of mutants appearing at the same time or almost at the same time ; (5) that mutations may follow very rapidly one upon the other, so that the complete

Plate 14.

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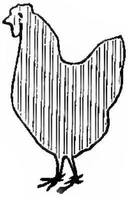

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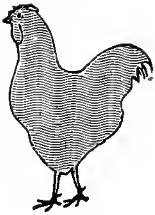

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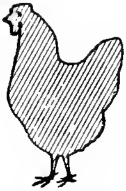

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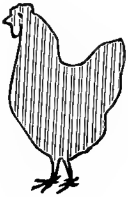

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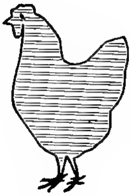

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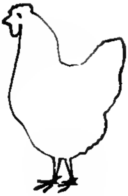

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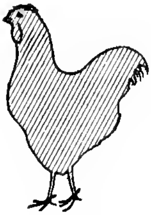

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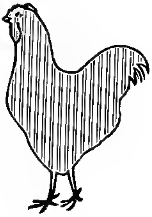

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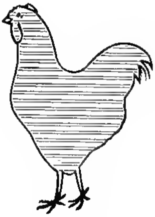

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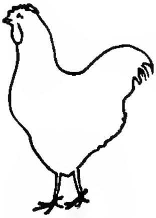

No 23.

Showing that Zero mated to L2 gives only Zero.

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mutation may take several years to complete itself ; (6) by a large " leap forward " the mutation may complete itself in a single generation.

This theory has none of the drawbacks attached to those of Darwin and Lamarck, and it has the undoubted merit of being supported by experimental evidence. Every breeder of any experience knows that so-called " sports " are quite common, and if he has ever mated these together, he knows that the new character is highly inheritable. The experimental evidence in support of the mutation theory is at our very elbow.

The L1 and L2 types have therefore arisen as mutations, that is, as " sports " from lower fecund types. The L2 is sometimes the result of a single mutation, and at others of two. Thus, L2 may arise by one " jump " as a mutation from Zero, as illustrated on Plate 5, but on the other hand, L1 may arise as a mutation from Zero, and later L2 may arise as a mutation from L1, so that L2 may be the result of two separate " jumps " instead of one. That both L2 and L1 are inherited, we have seen on Plates 7 and 10.

ZERO × L2. (*See Plate* 14).

Those who maintain that an L2 male *always* transmits L2 to his female offspring are likely to suffer disillusionment if they turn their attention to this mating, for, as shown on Plate 14, when a highly fecund L2 male is mated to Zero hens, *all* the progeny, quite irrespective of sex, is Zero. This information is of the greatest possible value to farmers and to others not possessing highly fecund stock, because it shows how frequently and easily they are misled by the Poultry Press.

The general farmer and the small backyarder do not, as we know, pay very much attention to the inheritance of fecundity. The stock is bred indiscriminately, and in too many cases the flock average is below—and in some cases I could mention considerably below—100. Owing to recent propaganda by the Press, lecturers, the Board of Agriculture, and by others, some of these people are at last beginning to realise that poultry keeping possesses far greater possibilities than they had at one time thought. They are beginning to seek the advice of so-called " poultry experts," and they are told to " buy males from hens with high records, to mate these to their hens, and that they will very soon find a marked increase in their egg returns." This advice is based on the entirely false assumption that a highly fecund male, *however mated*, will breed only highly fecund pullets. This assumption is altogether wrong; for if a highly fecund male be mated to Zero hens, all the offspring is Zero.

It is positively useless to breed from Zero birds, whether they be male or female, as, even when mated to highly fecund types, it will be found that all their offspring is Zero. Therefore the best advice that can be given to those with low fecund flocks of fowls is to " clear all out " and to make a fresh start with nothing but L2 types.

ZERO × L1. (*See Plate* 15).

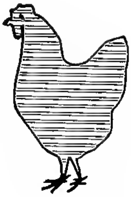
This mating is no worse than is the one we have just been considering, although it may appear worse on paper. Again, we find the Zero type dominant, which it invariably is, no matter how it be mated.

Before leaving this question of the inheritance of the Zero factor, it may be as well to point out that just as L2 may be " acquired," so may Zero be " acquired," and in such cases the birds breed *according to their genetical and not according to their acquired type.* In order that no mistake should be made on this point—which is most important—an illustration shall be given.

An L2 bird is a bird with a winter record of *over* 30 *eggs*; an L1 bird is a bird with a winter record of 30 *eggs or under*; a Zero bird is a bird *with no winter record.*

Plate 15.

PARENTS.



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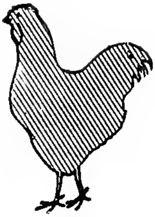

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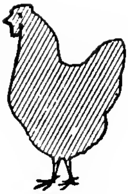
PROGENY.



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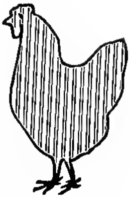

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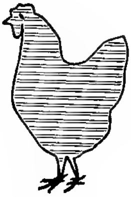

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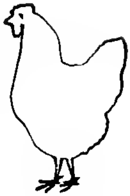

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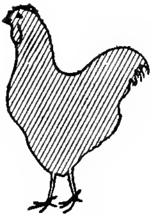

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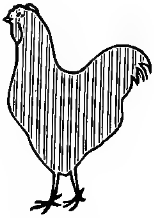

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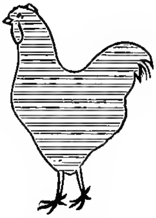

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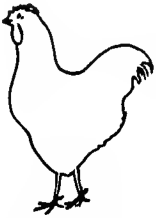

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Showing that Zero × L1 gives only Zero.

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Supposing a pullet to be hatched in January and to start laying in July, she will frequently fall into moult either in October, November, or in December. This moult, making a call upon the albuminous matter consumed, will modify the fecundity for the winter period, so that it would be quite impossible to correctly determine the genetical fecund factor carried by such a bird, since the unseasonable moult would be curtailing the production. If such a pullet laid 10 *eggs* during the winter months, she would not necessarily be L1 nor would she necessarily breed like an L1 bird ; if she gave *no eggs* during the winter months she would not necessarily be a Zero bird ; nor would she necessarily breed like a Zero bird. She will be, and will breed, according to her genetical and not according to her acquired type.

Conversely, the egg organs of a Zero bird may be stimulated to such an extent as to cause her to produce eggs during the winter months, but such fecundity is merely acquired by stimulating, and the bird will be found to breed according to her genetical type, which is Zero.

In studying the inheritance of fecundity, these points should be kept constantly in mind.

TYPE AND FECUNDITY.

There is commonly supposed to be what is known as a " laying type," although opinions differ considerably as to what actually constitutes this. We have two opposing schools of thought even among practical and experienced breeders. There are those who maintain that the type has positively no effect upon fecundation ; they point to such breeds as the White Leghorn and the Rhode Island Red, and they contend that although these two breeds are widely divergent in type, there are highly fecund birds of each. This school contends that birds of a so-called " ideal laying type " frequently prove but indifferent performers, while other birds which appear " most unlikely layers" often put up exceedingly high records. We must admit that there is at least some truth in all this, but exactly how much we shall consider in a moment.

The other school contends that there are certain characteristics peculiar to all really great layers, and that this combination of characteristics may be found in almost any breed and comprises the "laying type." The combination consists of:—

(1) Fine comb, long snaky head, and narrow skull.

(2) A very red and prominent eye; the more the eye "stands out" from the head the better.

(3) A long body sloping very gradually towards the tail; the tail itself being carried almost but not quite erect.

(4) The breast-bone very short, and the abdomen (which should be covered with the softest possible down) well developed.

(5) Legs rather above than below the average in length (but not too long); very fine and extremely pale in colour. The legs should be set wide apart.

(6) Toe-nails extremely short.

(7) Pelvis bones wide apart, not less than 2½ inches.

(8) Cartilage soft.

It is the *combination* of these eight characteristics which comprises the laying type—not any few of them by themselves.

First of all, it must be admitted that there are certain structural types which at least inhibit heavy fecundation, while ornaments of any kind are never found on a bird with great laying powers. Thus, the structure of Cochin, Brahma, Game, Malays, Aseels, short-legged-short-backed Orpingtons, and various other "fancy" types are never correlated with high fecundity. Then, turning to ornaments, these again are never found on highly fecund birds. Crest development, tail development, lobe development, development of feathers on the legs, cheeks, or throat, and similar abnormalities, are prejudicial to heavy production. There appears to be a great deal of evidence in support of the contention that certain " types " are at least opposed to heavy fecundation. But this is not quite the point. Is there a type correlated to high egg production, and, if so, what is the type ? and what is the nature of the correlation ?

In my experience the combination of those eight characteristics already set out is always present in a good layer, but—and this point should be noted very carefully—*this combination also exists in some bad layers.* We arrive, therefore, at two very simple conclusions : (1) That while all great layers possess certain characters in common, these characters are also possessed by other birds which are in no sense highly fecund. (2) That there are other characters positively prejudicial to highly fecund powers.

In so far that almost any utility breed may combine these eight characteristics, a " laying type " may be said to exist, but the point should not be pushed further than that.

When selecting the breeding stock to produce highly fecund birds, one should select first of all on the pedigree, secondly for the L2 factor, and lastly for a combination of those eight characteristics which we have called the " laying type." If we are able to make a satisfactory selection combining these three essentials, then we have an ideal mating.

SIZE AND FECUNDITY.

That size is correlated to fecundity there is do doubt whatever, but the correlation is not great, nor is it definite. You could not, for example, state it in explicit terms of so many pounds in weight, so many eggs, or anything of that kind. There are general rules upon which one may work, but even to these there are many striking exceptions, two of which we purpose giving in a moment.

The general rule is that the smaller the hen (within reasonable limits) the more highly developed are her fecund powers, and it is interesting to know how and why this is. There are three distinct views held by breeders.

The first and most common view is that bulk of body is diametrically opposed to egg production, because the material which would otherwise go to the formation of yolk is utilised in maintaining the bone and tissue. This view has no weight with scientific men, partly because the material required to build up bone and tissue (*i.e.*, to renew waste tissue) is in an adult bird so infinitesimal that it could not possibly absorb so much albuminous matter as to even slightly diminish egg production, and partly because, even were this not so, birds in their second and third seasons should (since the rebuilding of tissue is then greatly reduced) lay better than when in their first season, and the very reverse is usually the case.

The second view is that large birds are lethargic in disposition, and it is contended that unless a bird be active it will never prove a good layer. The latter part of this contention has never to my knowledge been disputed, and I think it is generally admitted that inactive birds are poor layers. But it yet has to be proved that a big bird is necessarily an inactive one. That some are, we may allow, but this appears to us to be more a question of strain and of breed than of size. Thus, we have known Light Sussex, Rhode Island Reds, and Croad Langshans quite as big as some Black Orpingtons, but they have been infinitely more active and much better layers. There appears to be a good deal of evidence to prove that lethargic birds are poor layers. but very little to prove that size causes lethargy. On the whole, then, we are not prepared to admit that large birds are poor layers because their size makes them lethargic —the assertion is too sweeping, and is not in keeping with the facts.

Lastly, there are those who contend that the selective breeding which evolves large birds, neglects their fecund powers ; therefore they are poor layers, not *because they are big,* but because they have been bred for size and not for eggs. If this be true, we should expect that if we bred for eggs and not for size that the size of the birds, *through being neglected,* would decrease. In our " bred-to-lay " Leghorns, Wyandottes, Rhode Island Reds, Orpingtons, and some Light Sussex we have heard of, this has actually proved to be the case, for the utility strains of these are very much smaller than is required by the exhibition standards fixed for these breeds. This view of size and its relation to fecundity we believe to be the correct one, because it is supported by experimental evidence. It is therefore claimed :

(1) That by selecting only the highest fecund birds for stock, it will necessitate neglecting size and the birds will, *for that reason,* revert to a smaller type.

(2) That by selecting only large birds for stock it will necessitate neglecting fecundity and the birds will, *for that reason,* revert to a lower fecund type.

Granting the correctness of this, it should not prove impossible by the most rigorous selective breeding to combine size with fecundity, and we know of several cases in which this has been done. One is that of an exhibition strain of Croad Langshans which has won highest honours in nearly all the classical events, the majority of the birds having splendid winter and yearly records. Another case is that of a strain of Buff Orpingtons (not exhibition), the pullets of which run from 7 lbs. to 8 lbs. each in weight, and with a flock average of just over 160.

BREED *versus* STRAIN.

The question is continually being asked as to whether heavy egg production is a matter of breed or of strain. Actually it is neither—it is a matter of specific breeding for high fecundity. It would, however, be idle to deny that both breed and strain play their part, and it would be folly to neglect either. Their relative importance differs at different times and under different conditions. For example, when we are considering the soil and climate in respect to the birds we intend for stock, the breed is of far greater importance than is the strain, but when once we have found the best breed for the environment, then it immediately becomes a question of strain.

Fowls are divided into breeds, varieties, and strains.

BREEDS are differentiated by structural characters.

VARIETIES are differentiated by colour and in domestic varieties of fowls, I fear, very often by lineage.

STRAINS are differentiated by lineage.

The breed and variety should be selected to suit the conditions under which the birds are to be kept, and it is impossible to exercise too much care in this matter. The following list may be taken as a safe guide :—

Conditions—HEAVY SOIL, DAMP DISTRICT, AND EXPOSED SITUATIONS. *Best Breeds to Keep*—Rhode Island Reds, Black Leghorns, Plymouth Rocks, Croad Langshans, Buff Orpingtons, White Orpingtons, Faverolles, Partridge Wyandottes, and Black Wyandottes.

Conditions—LIGHT SOIL, AND MILD, DRY DISTRICTS. *Best Breeds to Keep*—White Leghorns, White Wyandottes, Light Sussex, Silver Campines, Buff Rocks, Anconas, White La Bresse, Black La Bresse, Redcaps, Blue Leghorns, and Blue Andalusians.

Conditions—INTENSIVE HOUSES AND SMALL BACK-YARD RUNS. *Best Breeds to Keep*—White Leghorns, Black Leghorns, Silver Campines, Anconas, Blue Leghorns, and Blue Andalusians.

In this list we have confined ourselves only to such breeds as are capable of producing highly fecund stock. Now, the importance of breed, so far as egg production is concerned, can best be studied when viewed in relation to its environment. There are some breeds which lay much better in one environment than they do in another. There is a reason for this which may best be explained by two illustrations :—

(1) The Buff Orpington (that is, the utility Buff Orpington) is an excellent layer, but we find that she produces more eggs in the autumn and winter than she does in the spring and summer months. She is very prone to broodiness, and the warm weather from April to September encourages this, and that checks her production. On the other hand, she has an abundance of soft downy feathers, which protects her against inclement and cold weather, so that she is able to lay well under conditions which would entirely check the production of a more sparsely feathered bird. So it comes about that, if properly managed, she lays better under severe climatic conditions than she does under what would at first sight, and without careful examination of the facts, appear far more favourable circumstances.

(2) But the White Leghorn is a sparsely feathered bird, is usually non-broody, and is capable of high productivity. If this breed be placed under rigorous climatic conditions it is not to the same extent physically adapted to meet them as is the Buff Orpington for example, and the result is that it seldom if ever lays well under conditions which would, by checking broodiness, encourage egg production in a more heavily feathered breed. On the other hand, the White Leghorn and similar breeds are not rendered excessively broody (even if they go broody at all, which they very seldom do) by warm and favourable conditions. And so it comes about that on a light soil, and in a dry, warm climate, the White Leghorn will give its maximum return of eggs, although under rigorous conditions it will not lay so well as will a less highly fecund but better protected breed.

We therefore see that in respect to the conditions under which the birds are to be kept, breed is of primary importance.

But after selecting the best breed for our locality, we then have to turn our attention to strain, and this is a subject worthy of the most careful consideration.

All families (strains) of even the same breed are not of equal value as layers, as there are good, medium, and bad fecund strains of all utility breeds. There are several conditions under which any breed can be bred ; it can be selected generation after generation for (1) size, (2) colour, (3) exhibition, (4) table qualities, (5) the L2 factor, or (6) there may have been no selection at all, the birds being allowed to breed among themselves more or less indiscriminately.

Strain, then, assumes an aspect of great importance, for we require not only birds of a suitable breed, but also those which are descendants of a line which have been bred generation after generation from nothing but L2 birds. Breed does not ensure this.

VALUE OF THE CYCLE.

We now come to a matter of considerable importance, but one which is woefully neglected even by many scientific breeders : we refer to the egg cycle.

In trapnesting a hen, her eggs are entered up daily on a Recording Sheet, and all that we trouble about is the total number of eggs laid in each month, the winter record, and the total for the year. This is not enough. We require to know in what sequence the eggs are produced, as this has an important bearing upon the building up of a highly fecund strain. Let us consider the various points of interest.

Batches.—In a wild state the hen lays from 10 to 15 eggs ; she then stops laying, incubates these, rears the resulting chickens up to eight or ten weeks of age, and then lays another 10 to 15 eggs and goes through the same process of hatching and rearing. This is usually done twice in a year, so that two lots of from 10 to 15 eggs are laid and two broods of chickens are reared. Each 10 to 15 eggs separated by a period of hatching and rearing is called a " batch " or "clutch." In the domestic hen eggs are also laid in " batches " or " clutches," but are not always intercepted by periods of broodiness. These batches are a survival of the wild habit, and are of the greatest possible importance.

Two points should be most carefully noted in this connection :—(1) The size of the batch itself, which varies from about 10 to sometimes over 100 eggs in individual birds. (2) The length of the period separating one batch from another, which varies from a few days to several weeks in some cases. In addition to selecting for L2, we must select pullets for stock, giving big batches with short periods intervening between one batch and another.

Sequence.—Fowls show considerable variation in the sequence of laying, the following being typical examples :—

(1) Lay 1 ; miss ; lay 3 ; miss ; lay 2 ; miss ; lay 3 ; miss.

(2) Lay 5 ; miss 3 ; lay 5 ; miss 2 ; lay 5 ; miss 2 ; lay 5 ; miss ; lay 4.

(3) Lay 3 ; miss ; lay 4 ; miss ; lay 4 ; miss ; lay 6 ; miss 2 ; lay 3.

(4) Lay 10 ; miss ; lay 16 ; miss ; lay 10 ; miss 2 ; lay 3.

(5) Lay 20 ; miss 21 ; lay 18 ; miss 14 ; lay 21 ; miss 10 ; lay 8.

(6) Lay 5 ; miss ; lay 6 ; miss ; lay 5 ; miss ; lay 7 ; miss ; lay 6.

Now this sequence is of great importance, for we almost invariably find that the *shorter* the " misses " of a hen and of a male's dam the better will the *flock average* of their daughters be. Thus, a bird missing one day between every four to seven eggs, will be found to give much better stock than will a bird in the habit of laying from 18 eggs upwards in as many consecutive days, and then laying none for the next fortnight or three weeks. The steady and consistent layer is far better for stock than is the bird which never misses when she is laying, but indulges in long " rests " between every 20 eggs or so, even although both may put up the same yearly total.

Weather.—If on our Record Sheet we keep a daily note of the weather, we shall find that the egg cycle of some hens is more severely affected by any changes than are those of others. Both susceptibility and non-susceptibility to weather changes are highly inheritable, so that the consideration of this point is one of no little importance. If we can—as is most certainly possible—produce a strain whose fecundity is not affected by bad weather, that in a large measure ensures us against loss in a wet or cold spring and summer.

RECORDS AND SELECTION.

There is a grave danger of breeders giving too much attention to the yearly and not sufficient to the winter records of their stock. It is perfectly true that the sum total of eggs laid in the entire year is of greater immediate importance than is the number laid during the three months of the winter test Nevertheless, since the yearly records of each generation must depend on the wise selection and mating of the parent stock, we cannot afford to ignore this side of the subject. It very frequently happens than an L1 bird puts up a good yearly record (perhaps a much better one than that of some of the L2 birds), and on that account she is selected for stock. This is a mistake due to thinking that the *yearly record is inherited*, which it is not.

We very frequently hear of birds giving " much better layers than themselves," and also of " very good layers giving very bad layers." This is very easily explained either on the hypothesis of faulty matings or of faulty selection. Let us give cases illustrative of both :—

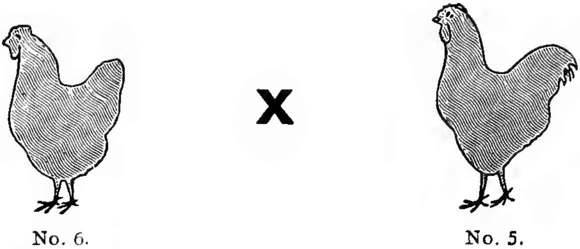
(1) A bird with a winter record of 10 and a yearly record of 180 eggs when mated to an L2 male gives mostly bad layers.

(2) A bird with a winter record of 50 and a yearly record of 140 eggs when mated to an L2 male gives only good layers.

(3) A bird with a winter record of 60 and a yearly record of 200 when mated to an L1 male gives only poor layers.

Plate 16.

PARENTS.



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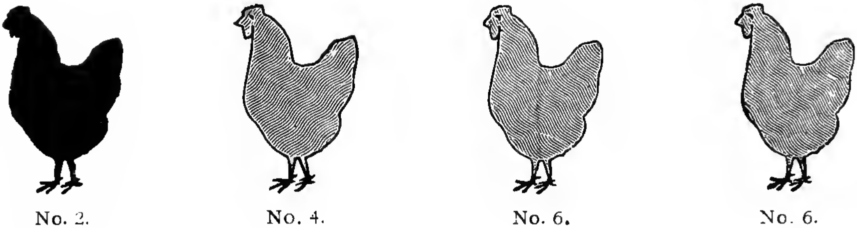
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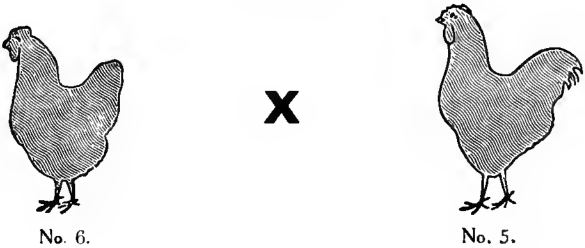

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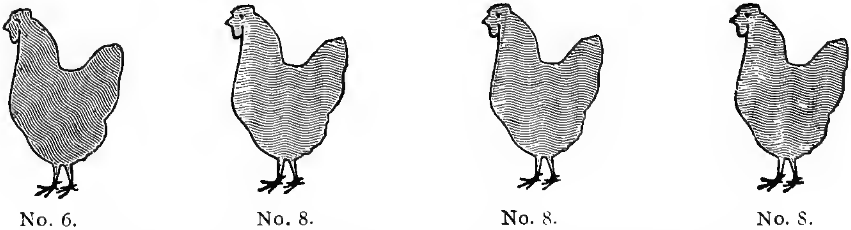
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No. 8.

Showing "progressive" (top mating) and "retrograde" (bottom mating) variation.

To face page 49.

These three points illustrate how " bad layers are bred from good layers," and also how " good layers are bred from moderate or poor layers." They illustrate both bad selection and bad mating (except (2), which is quite good), and emphatically show that the winter record is the only record possessing any selective value.

But there is a further point to consider in respect to records and selection. Even among L2 birds we find the greatest possible variation in both their winter and yearly records. Have the highest variants any selective value not possessed by lower variants ? To put the question in a more concrete form : Would L2 birds with a winter record of 70 and a yearly record of 250 breed any better layers than would birds with a winter record of 50 and a yearly record of 220 ?

Actual experiments have shown the difference to be so slight as to be almost negligible. As a rule—but by no means invariably—the higher L2 birds give an odd higher record variant than do the lower record L2 birds ; but the *flock average* of the daughters of both is substantially the same.

And yet since some strains of L2 birds have higher yearly records than have others, all cannot possibly possess the same breeding value. And since this is so, some L2 variants must have a selective value peculiarly their own, even if this is not to be found in the winter or yearly records. Let us see if we can find any basis for this super-selection.

PROGENY AND SELECTION. (*See Plate* 16).

The value of a stock bird consists in what it will breed ; and that is the standard by which it must ultimately be judged. However well things may work out on paper, the final, and indeed the only, test is *What the Birds Will Breed.*

On Plate 16 we have shown one of the commonest phenomena in breeding highly fecund fowls. Two matings are shown ; in both cases the males and females used carry exactly the same fecund factors, namely, a winter record of between 40 and 49 and a yearly record of between 200 and 229. Apparently, and so far as it is humanly possible to judge with the means at our disposal, the parents in both cases are exactly the same and should therefore give exactly the same class of fecund offspring. But they do not. The female offspring in the top mating show what is known as a " progressive " fecund tendency, that is to say, that the *worst* layers are equal to their parents, while the *best* are far superior. Quite half the female offspring is better than either parent, while the other half is about equal to the parent stock. The female offspring from the bottom mating is quite different to that from the top mating; they show what is known as a " retrograde " fecund tendency, that is to say, that the *best*—barely 25 per cent.—is only equal to its parents, while the remainder is much inferior.

These facts are remarkable, but well established. In all characters in all domestic races the progressive or the retrograde tendency is often most marked, and here we possess a test, if properly applied, of high selective value.

Both the progressive and retrograde tendencies are highly inheritable. Were it not so, their selective value would, of course, be *nil*. These tendencies run in both strains, and in individuals, and are probably associated with the protoplasm. Very frequently one part of the strain will be progressive, while another part of the same strain will be retrograde. This remarkable phenomenon is commonly met with and is seen at its best when the same strain is in widely separated yards.

The progressive strain or individual can only be determined by comparing the fecundity of the female progeny with that of its parents, and it is a wise plan to select for stock only those birds bred from parents in which the progressive fecund tendency is most marked.

Another point to remember is the value of a pen of breeding birds throwing as shown in the top mating on Plate 16. Such birds are worth keeping so long as they will breed chickens.

MATURITY AND SELECTION.

Many breeders have a very bad habit of selecting their birds—and especially their cockerels—on the rapidity with which they mature, under the mistaken assumption that the *earlier* a bird matures the better layers will it breed. The cockerels which crow first, the pullets which shoot their combs first, are all supposed to possess a selective value over and above that of their fellows. All this is quite wrong. Cockerels which crow at eight weeks and pullets which start laying when only 4½ months old, so far as fecundity is concerned, possess no selective value whatever. In fact, they are dangerous birds to breed from.

But, putting aside the question of precocity, the date of maturity is of some importance, and is well worthy of consideration.

Birds of each breed should mature as near as possible at the normal time for the breed, which is variable, as the following list shows :—

Leghorns, Campines, Anconas, Andalusians and Mendels	6 to 6½ months.
Buff Rocks, Minorcas, Sussex, Wyandottes, Rhode Island Reds, Redcaps and La Bresse	6½ to 7 months.
Orpingtons and Plymouth Rocks	7 to 8 months.
Langshans and Faverolles	7½ to 9 months.

Now it must be obvious that you can have two kinds of maturity variants—those that mature earlier than the normal period for the breed, and those that mature later. It seems unnecessary to state that both of these are faults which should be guarded against. If we select for stock birds that mature earlier than is normal for the breed, sooner or later the *size of the egg* will suffer and the strain be ruined in this way. If, on the other hand, we breed with birds which are rather late in maturing the size of the egg will *increase*, but sooner or later the fecundity will suffer and the strain be ruined in this way.

As regards maturity then, the nearer we get to the normal for each breed the better ; but for very late maturing breeds—such as Langshans, Faverolles, and some strains of Black Orpingtons and Plymouth Rocks—to get them, by selective breeding, to mature a little nearer 7 months would be a distinct advantage from the utilitarian standpoint.

HOW TO BREED COMPETITION WINNERS.

The Laying Competition, although, to my mind, having little to commend it as now constituted, yet possesses a great attraction for many breeders of highly fecund stock, and it cannot be denied that a win in one of these competitions invariably proves an excellent advertisement.

It should be pointed out that in order to meet with any measure of success the birds should put up an average of well over 200 eggs each. Now, the competing pullets are usually sent away in October, and as the birds may or may not have started laying by then, it is almost impossible for the breeder to select a pen of, say, six pullets which he *knows* are going to average well over 200 eggs each. And yet that it is not *quite* impossible is proved by certain breeders' birds doing well in every Laying Competition in which they are competitors. And this kind of thing goes on year after year. This, if it proves nothing else, at least shows that successful competition must be largely a matter of breeding and selection.

Since it be essential that the birds entered should be most highly fecund, the first point is that nothing whatever should be left to chance, and it is safe to contend that the element of chance is bound to exist unless we are dealing with the offspring of a tested mating. This point should be obvious, but it is not. Too many breeders who essay to win in a Laying Competition rely on either (1) L2 hens mated to an untested cockerel out of a highly fecund dam, or (2) L2 pullets mated to an untested cockerel out of a highly fecund dam. Both matings by their nature are unproved ones, and are therefore unsound.

In the first place, however highly fecund the dam may be, you cannot say whether any individual son of hers carries L2 or L1, *unless* you have tested and proved him. We have already shown the female progeny from L2 × L2 and from L2 × L1 on Plates 7 and 8, and, as will be seen at a glance, if the male be an L1 type (as many males from highly fecund hens are), then his female offspring would be quite worthless for a Laying Competition. You are compelled to take this risk whenever you use cockerels for stock, and, of course, the mistake is not realised until the bird's daughters have been in the competition for some time and have failed. The first point then, is to rely on no pullets that have not been bred from proved L2 parents on both sides.

But that is not all. Pullets bred from proved L2 parents on both sides are not necessarily fit to win a Laying Competition, as many L2 birds (see Plate 1) have not sufficiently high yearly records. If we turn to Plate 16, we have there an illustration of both progressive and retrograde fecund variants from *inter se* L2 matings. Now, it can be explicitly stated that the *same pen* gives the same class of progeny year after year for as long as it be capable of producing chickens, and here we have the most important point in the whole subject. Breed the pullets you intend entering in a Laying Competition from two or three-year-old birds (both male and female), which proved in their first breeding season that they give only highly fecund pullets. It frequently happens that *one* bird in the pen gives in her pullet year much higher fecund variants than did any other pullets mated to the same male. This is quite common, and in a case of this kind the pullet and cockerel should be moulted out early so that as second season birds they may produce in March or April (according to the breed and the normal age at which it matures) a sufficient number of pullets to allow of some selection in the following Autumn for the Laying Competition.

Do not rely on the progeny of untested birds to provide you with pullets suitable for competing in Laying Competitions. Rely for selection on the progeny of second season birds which in their first breeding season gave only highly fecund pullets.

Selection then becomes an easy matter, for it resolves itself into selecting the best from a bunch of pullets, all of which you know will prove good. A mistake, under such circumstances, cannot possibly prove serious, as all risk of having bred any inferior birds has been eliminated. The final selection should be for a combination of those eight characteristics of a good layer given under *Type and Fecundity.*

HOW TO IMPROVE THE FLOCK AVERAGE.

The flock average is of the greatest possible importance, because on it depends the success or failure of the poultry farm. Money is neither made nor lost on a few odd birds with abnormally high records, but it is made or lost on the egg average of the entire flock. For breeding purposes, as we have already shown, everything depends upon the individual record, but the individual record counts for nothing when the income depends wholly or partly on the sale of new laid eggs. To ensure success in this business the flock average must be not less than 150 eggs per bird, and that is absolutely the lowest average on which we can successfully work.

On an egg farm, where a heavy head of stock has to be maintained, trapnesting is usually too great a tax upon labour to render it feasible. At the same time a good flock average cannot be attained unless the best birds be bred from.

Usually one big breeding pen of upwards of 100 pullets and hens is run ; a number of males are turned down among these, and from 500 chickens upwards are reared annually from this one flock. The question arising in an undertaking of this kind is how to select the stock birds with judgment, for it is clear that this is a most important subject.

After spending many years on this most difficult of all problems, the following has been found at once the simplest and the soundest scheme :—

In September, house separately well-developed March and April hatched pullets which have not at that time commenced to lay. Feed them as advised under *The Basis for Selection.* During the month of October put a *Red* leg band on the leg of any pullet you find on the nest ; in November put a *Green* leg band on the leg of any pullet you find on the nest ; and in December put a *Black* leg band on the leg of any pullet found on the nest. Although pullets frequently go to the nest and do not lay, this will nevertheless prove a good guide, and it entails very little trouble. Select in January those pullets wearing three leg bands—a red, a green, and a black—and the vast majority of these will have been laying during the months of October, November and December, and nearly all will be L2 birds. This is the best method of selecting the breeding pullets without the aid of the trapnest. Each generation the pullets should be selected by this method. In the event of an insufficient number of birds being found with three leg bands, select those with a green and a black, but never those with two other colours, as either red and green or red and black indicates by January that the bird missed one month in three after starting to lay, and she is likely to be an L1.

It is not advisable that any stock males should be reserved from pullets selected in this manner, as the stock males *must be* from trapnested record dams. The males should be purchased each year, and this should be done with the utmost care. Let us give a list of the chief points to remember :—

(1) Purchase from the same breeder each year.

(2) See that this breeder's farm is situated *as far away from your own as possible,* as the greater the distance from which the males are brought the better.

(3) See that the males are bred from hens with a winter record of not less than 50 and a year's record of not less than 200 eggs.

To select and to mate pullets and cockerels on this principle will be found thoroughly sound in every way, as it will markedly increase the flock average up to about 180 eggs. It will not increase beyond that, nor is it necessary that it should.

L(2) AND L(1).

When pullets are hatched in December, January, or February, it very frequently happens that they start laying in July and continue to do so until October or November, when they fall into a partial moult and laying ceases until the end of December or the beginning of January, when they usually start again. It is obvious that such birds cannot be expected to pass the test for L2, not because they are necessarily L1 or Zero birds, but because of them being hatched so early in the season, which has been largely responsible for them falling into moult just about the time when the winter test is starting. Again, late hatched birds are not sufficiently matured to be in lay during the winter test, and consequently they also cannot be tested for the L2 factor.

It is at least presumable that many of the birds in these two classes would have passed the L2 test had they been hatched in March or April, and their failure to do so is frequently the result of circumstances rather than of heredity. Some of them may conceivably have inherited a highly fecund factor, and it remains necessary to find a standard by which they may be tested.

The Scientific Poultry Breeders' Association of England has drawn up such a standard for testing in months other than October, November, December, and January, but it has (wisely, I think) differentiated between birds passing a test in months other than those given by calling such birds L(2) instead of L2, the brackets round the figure signifying that the test passed was not a winter one. The Continuous Testing Table drawn up by this Association is given below :—

Test Starts.	*Terminates.*	*Eggs During Test.*	*Classification of Pullets.*
Jan. 1.	March 31.	60 or over.	L(2).
Jan. 1.	March 31.	Under 60.	L(1).
Feb. 1.	April 30.	60 or over.	L(2).
Feb. 1.	April 30.	Under 60.	L(1).
March 1.	May 31.	65 or over.	L(2).
March 1.	May 31.	Under 65.	L(1).
April 1.	June 30.	65 or over.	L(2).
April 1.	June 30.	Under 65.	(L1).
May 1.	July 31.	65 or over.	L(2).
May 1.	July 31.	Under 65.	L(1).
June 1.	Aug. 31.	60 or over.	L(2).
June 1.	Aug. 31.	Under 60.	L(1).
July 1.	Sept. 30.	50 or over.	L(2).
July 1.	Sept. 30.	Under 50.	L(1).
Aug. 1.	Oct. 31.	40 or over.	L(2).
Aug. 1.	Oct. 31.	Under 40.	L(1).
Sept. 1.	Nov. 30.	40 or over.	L2.
Sept. 1.	Nov. 30.	Under 40.	L1.
Oct. 1.	Dec. 31.	30 or over.	L2.
Oct. 1.	Dec. 31.	Under 30.	L1.
Nov. 1.	Jan. 31.	40 or over.	L2.
Nov. 1.	Jan. 31.	Under 40.	L1.
Dec. 1.	Feb. 28 or 29.	60 or over.	L2.
Dec. 1.	Feb. 28 or 29.	Under 60.	L1.

These tests are started on the first of the month following the laying of the first egg.

There are certain points in respect to this testing for L(2) which should not be overlooked.

In the first place these tests should not be applied to birds which were hatched at a time of the year which made them old enough to pass the L2 test, but which in that test came out L1 or Zero. To neglect this point is to risk including inferior birds in the breeding pens, and the breeder himself will have to suffer most for such an error of judgment.

In the second place, birds which are very late in maturing should not be subjected to this L(2) test, as if they pass it (as they well may), it will mean introducing a late maturing factor into the strain, which, as we have already seen, is inadvisable and unprofitable.

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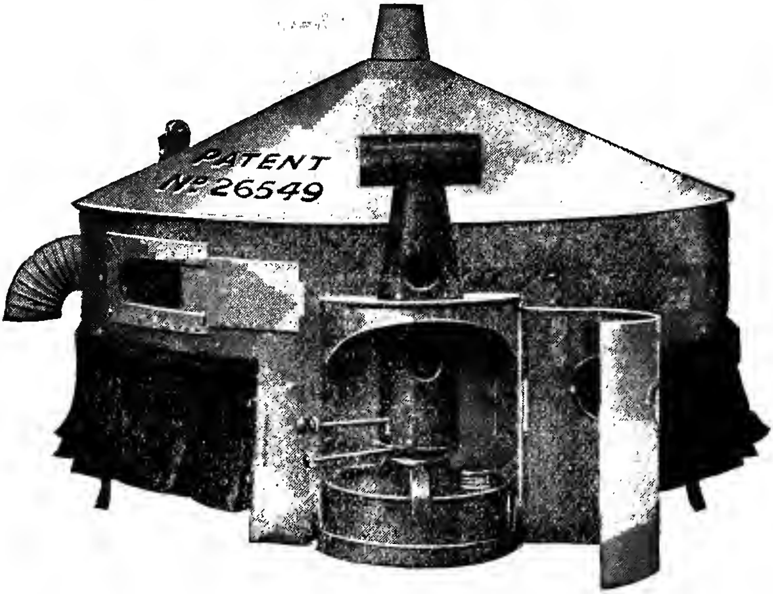

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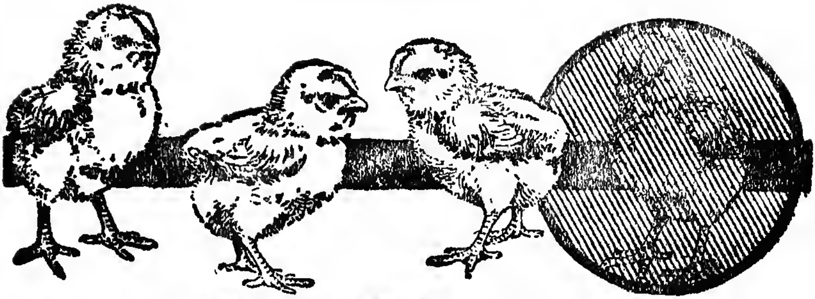

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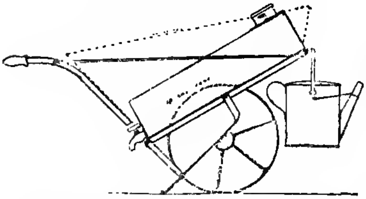

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